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LEARNING AND RETENTION OF VERBAL MATERIALS [1]

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I. General Theory

Introduction. Recognition of the theoretical importance of the concepts of learning has been steadily increasing and in the work of the last four years upon the learning and retention of verbal materials this recognition shows a positive acceleration. Early in the history of experimental work upon these problems there was a tendency to regard them as being primarily practical, of importance to the teacher

[1] This review covers the period since the preceding review by the writer, "Memory," Psychol Bull., 1930, 27, 514–563. The scope of the present review has been widened to include theoretical formulations of the problem of learning and discussions of the "laws" of learning, all of which have traditionally been treated under the head of *the acquisition of skill* but which are equally relevant to *memorizing*. The boundaries between *skill* and *memory*, as the two are treated in the experimental literature, are too unclear to have more than an arbitrary and transient usefulness. This review has been limited to work with verbal materials which are learned as presented, *i.e.*, in the learning of which elimination of errors and the necessity of discovering and selecting the correct acts tend toward a minimum. This criterion has been transgressed in a few cases where an interpretation of the data seemed to justify it. In order to keep the review and bibliography within the space available, it has been necessary to omit more than 100 titles, but an attempt has been made to include a representative selection of titles and a complete list upon some of the more active problems. Any such selection must, admittedly, be weighted by the bias of the reviewer, and omission of a paper does not mean that the paper is valueless. The review aims to give a brief summary of the current of research during the four years. There is not space for factual detailed criticism, or for mention of every paper.

but not to the systematic psychologist. Even the results of Ebbinghaus and of G. E. Müller were discussed more often at the level of the educational psychologist than at that of the general psychologist. Slowly the perspective has been changing until now the concepts of learning are, to a large number of psychologists, among the most pervasive of the concepts of psychology.[2] We shall begin this review with a discussion of general problems.

Constancy Attitude Versus Relativity Attitude. In a paper of the first importance for both theory and experiment Carr (30) has contrasted two attitudes toward experimental problems. The older and still the more common is the *constancy attitude* which assumes that psychological phenomena are constants and that the variations which measurements show are the outcome of masking conditions which conceal or distort the "true" values. Experimenters dominated by this attitude search for single or absolute curves of learning and retention, for "true" memory spans and for many other such "true" phenomena. Opposed to this is the *relativity attitude* which assumes that any given phenomenon is a function of N variables and seeks to find what these variables are and to measure their influence. A given phenomenon may be affected by some variable conditions and not by others, but only when we adopt the relativity attitude can we expect to work out adequate scientific descriptions[3] in which both variables and constants appear in a measured perspective. That the relativity attitude is becoming more prevalent in work on learning is, to the reviewer, one mark of the scientific coming-of-age of the psychology of learning.

Descriptive Formulations of Learning. There have been, during the four-year period, a number of restatements of what is meant by learning and of what its problems are. Thorndike, in *The Fundamentals of Learning* and in a series of papers, has defended a "new connectionism," less rigid than the old and extended by such new concepts as belongingness, identifiability and system until the older

[2] Klein (110), for example, cites learning as the fundamental criterion of differentiation between psychology and physiology.

[3] *Cf.* Dodge's (54) suggestion that a newer psychophysiological generalization, "Without variability, no mind," should be substituted for the old dictum, "Without phosphorus, no thought." He adds: "But the variations must apparently be of definite and distinctive kinds, connected in a specific manner with systematizations of relative persistency." *Cf.*, also, H. M. Johnson's "Some Follies of Emancipated Psychology, *Psychol. Rev.*, 1932, 39, 293–323, in which he points out certain cases of generalization in learning without regard to the restrictive conditions.

connectionist system has almost vanished.[4] Rexroad (**181**) reinterprets the conditioned response to be, not the form to which all other cases of learning reduce, but as revealing "the primary and secondary factors involved in all learning." In the context of conditioned response descriptions Loucks' (**125**) critical appraisal of Pavlov's systematization of behavior should be read.

Opposed to these two descriptions in associationistic terms are the more configurational formulations of Adams (**2**) and Humphrey (**94**).[5] For Adams learning is the organization or reorganization of a field distorted by an obstructed need, while Humphrey sees it as a series of organic acts in which later terms presuppose earlier ones and which tends toward an optimal term from the point of view of systemic conservation.[6] Hollingworth (**89**) argues that learning is cue-reduction and Pyle (**175**) replies that ideational learning cannot be described as cue-reduction and can be handled adequately by a bond hypothesis. Of interest, also, is Rashevsky's (**178**) attempt to deduce from kinetic and thermodynamic principles the possibility of the existence of physical systems which exhibit learning.

It is a significant commentary upon the generality of learning that Boring's *Physical Dimensions of Consciousness* (**17**), although explicitly dealing with it in only a few pages, adverts to it in contexts in which it has often been conceded no place. However learning may occur, it is, he asserts, "a process of getting into a single consciousness more than it would originally hold," while consciousness "depends upon memory for our knowledge of it." Boring offers some interesting prolegomena to a physiology of learning and Dodge (**54**), who in *Human Variability* speaks of learning as "an increased sensitivity of particular systems to restimulation," deals through a large part of the book with the physiological conditions of the organization which learning means.

In one way or another each of these descriptions fits the facts, but none attains the comprehensiveness which a thoroughgoing systematization of the facts requires. These contemporary discussions are, however, vastly more critical than those of a decade or two ago and

[4] *Cf.*, Wyatt's (**242**) critique of the Thorndikian concepts of the period prior to the one covered by this review.

[5] See, also, R. H. Wheeler and F. T. Perkins, *Principles of Mental Development*, New York: Crowell, 1932, and the review of it by the present writer (**146**).

[6] The reviewer has elsewhere questioned this descriptive criterion. *Cf.*, PSYCHOL. BULL., 1934, **31**, 220–222.

they should lead further. In the meantime it is well for experimental work that we can formulate problems and interpret them at the level of experiment in terms of operational concepts. Thereby we may hope to gain the data which will permit system.

Association Versus Gestalt. Somewhat apart from these restatements of the problem of learning are the specific treatments of associationism and of *Gestalt-psychologie.* Carr's (**29**) paper and Robinson's (**183**) book are the two most important discussions of the former. Each exemplifies the relativity attitude toward the problem and each, by an explicit statement of the associationism implicit in experimental work advances both theory and experiment. The papers by Dugas (**61**) and Fauville (**66**) are also of interest here.

Associationism does not, however, hold the contemporary field unquestioned. Line (**120**), in an interesting paper, reviews the bearing of the work of Köhler, Lashley and Spearman, all of whom are presented as antagonists to associationistic atomism. Spearman, particularly, is held to offer an adequate substitute. Important as is the work of these three men, one may question whether it is necessarily opposed to the associationism referred to in the preceding paragraph, for both that and the later Thorndikian version are far from being atomistic. Harrower's (**82**) paper is a vigorous exposition of Gestaltist principles in learning and, especially in memory.

The Primary Conditions of Learning. The law of effect has more often been discussed in connection with selection and elimination than with the fixation of presented terms. Such fixation requires, however, determining conditions and the work on effect is as relevant here as in cases of explicit trial-and-error. The extensive experiments by Thorndike and his collaborators are well known.[7] They leave little doubt, had doubt been defensible before, that the effect of a connection influences the fixation of that connection. They also attack the problems of the spread of the influence of reward, the relative influence of rewards and punishments, the influence of irrelevant rewards, the modes of operation of effect and other related ones. Both experiments and conclusions are too numerous and complex to be reviewed in detail.

The experiments at Teachers College have already elicited a large

[7] Thorndike (**209** to **215**, inc.), Thorndike and Rock (**216**), Lorge (**122**, **123**), Lorge and Thorndike (**124**). See, also, E. L. Thorndike and G. Forlano, "The Influence of Increase and Decrease of the Amount of Reward Upon the Rate of Learning." *J. Educ. Psychol.*, 1933, **24**, 401–411.

amount of discussion. Trowbridge and Cason (219) have partially repeated one of Thorndike's experiments and, while corroborating it in essential fact, have interpreted their results as opposed to a backward action of effect.[8] Stephens (199, 200) proposes a reinterpretation of the law, Ogden (168) gives it a Gestaltist interpretation, and Boring (16) suggests two possible alternatives to a backward action of effect. Cason (31, 32, 33), who interprets the law of effect as a form of pleasure-pain theory, discusses that theory historically and critically and rejects it.[9]

The critics of the law of effect have for the most part taken effect to mean pleasantness and unpleasantness. Thorndike, it is true, speaks of the satisfyingness and annoyingness of a connection and of the backward action of these two. His experiments, however, can as well be interpreted to measure the influence of *knowledge of results* and of whatever non-affective concomitants such knowledge has, and Thorndike often writes as if he meant such non-affective effects. To the extent that effect is interpreted in this way arguments concerning pleasure and pain are beside the point.

Many of the experiments to test the law of effect have also tested frequency or have added other possible factors. As a result frequency has come to be regarded as an important determiner of learning only indirectly and by virtue of the fact that it acts as a carrier of effect and of other conditions. The experiments cited, together with others, have received a critical examination in a paper by Hunter (95). He concludes that the two fundamental factors are unconditioned or well-established conditioned responses and time. Effect, belongingness, frequency and other conditions are subsumed under these two fundamental ones.

The concern with the primary determiners of learning shown in these and many other papers during the four years represents a distinct advance.[10] With it has gone an increased interest in theories of memory, which until recent years have been largely disregarded by experimentalists. Even yet the four sets of problems, the descriptive characteristics of learning, its primary determiners, the descrip-

[8] Tolman, Hall and Bretnall (218) have obtained evidence in the learning of a trial-and-error problem which they interpret as opposed to the law of effect and in support of three substitute laws. See, also, Goodenough (75) and Tolman (217).

[9] On the history of the theory, see also, Pyle (176). On the Thorndikian position in general see Hsiao (91).

[10] See, also, Muenzinger (166), Humphrey (94), Restorff (180), Rexroad (181), Adams (2), Wyatt (242), *et al.*

tive properties of retention and its primary determiners are treated too commonly as district and compartmented, whereas it seems reasonable to suppose that each implies the others at least to a considerable degree.

Theories of Memory. In this field Bartlett's (**11**) volume on *Remembering* is an outstanding contribution. As an outcome of a large number of experiments on repeated and serial reproduction, all of which are important in their own right, he comes to the theory that "remembering is not the reëxcitation of innumerable fixed, lifeless and fragmentary traces. It is an imaginative reconstruction, or construction, built out of the relation of our attitude toward a whole active mass of organized past reactions or experience, and to a little outstanding detail which commonly appears in image or in language form." Related to this are Hillebrand's (**88**) essay toward a theory of memory in which he deals with such questions as immediacy, temporal dating, personal reference, and their carriers and Lewis' (**119**) note on the doctrine of memory-traces, in which he holds that the term 'trace' is useful only as a fictional concept to bridge the gap between impression and recall.[11]

There is, in many of the papers on the problem, a trend away from a physiological 'trace' theory of memory, all attempts at which have, according to Fischl (**67**), failed. In its place are offered various substitutes which do not lend themselves to brief characterization. These theories counteract the naïve oversimplicity of some of the 'trace' formulations, but most of them so far forsake the empirical data as to be of doubtful positive value. They do, however, bring the problem into sharper focus and should provoke experimentation.

Theories of Forgetting. The waning of measurable retention shown by the traditional curves of forgetting has too often been left without attempt at empirical explanation or has been referred to disuse in a way which amounts to little more than a restatement of the facts. Almost no attention has been given to the interrelations of theories of learning, memory and forgetting. Lepley (**116**) has, however, published a theory of serial learning and forgetting, based on conditioned response principles, in which he attempts to deal with both learning and forgetting and to account for the form of curves of forgetting. The relations of refractory phase and retention have

[11] See, also, Ach, Gerdessen, Kohlhagen and Margaritzky (**1**), Boring (**17**), Carmichael, Hogan and Walter (**28**), Crile (**44**), Grant (**76**), Johannes (**103**), McGilvary (**154**), Restorff (**180**), Roters (**185**), Scola (**188**), Stern (**201**), and Wohlgemuth (**239**). Of interest in this context are the papers by Delacroix (**50**) and Dugas (**60**) on the problem of affective memory.

been interestingly discussed by Dodge (**54**), and Lundholm (**132**) has dealt with forgetting in a tentative theory of functional amnesia. McGeoch (**144**) has summarized the evidence in support of inhibition from interpolated activities and altered environmental stimulation as the two major determiners of forgetting. The papers on theories of memory and of learning have occasionally dealt with forgetting in an incidental fashion. A theory which could satisfactorily relate the three would go far toward unifying the general field.

II. Experimental Work: Learning

Methodology. Experimental work on the learning of verbal materials proceeded for a long time with little explicit regard for calibration of materials or for standardization of methods and conditions. It is, as a result, often difficult to compare and evaluate the results of different experiments even upon closely related problems. During the last few years, however, there have been several methodological studies. Davis (**47**) and Sauer (**186**) have measured the relative variability of nonsense syllables and of words both from list to list and individual to individual and have shown that in several respects Ebbinghaus' original assumption of the superiority of syllables as learning materials is unjustified. Coördinate results have been obtained by McGeoch (**140**) in a study of the variability of lists of syllables of different associative value and of words.

Stroud, Lehman and McCue (**205**) have shown that syllable lists have fairly high reliability coefficients (0.60 to 0.80) and several other papers have incidentally reported coefficients of similar height for verbal materials. Hull (**92**) and Krueger (**114**) have reported upon the relative meaningfulness and difficulty of nonsense syllables. Barr (**9**) and McGeoch (**147**) have found that different experimenters, working under similar conditions obtain closely similar results.

Curves of Memorization. The work on this problem has gone on from the forms of memorization curves to a study of the conditions under which each appears. White (**230**) has found a clear end-spurt in the curves for several different materials. Simley (**191**), in a monograph of considerable theoretical importance, reports that learning above the threshold is continuous with learning below the threshold and that the form of the supraliminal portion of the curve is a function of that of the subliminal portion. McGeoch (**148**) finds that the Kjerstad-Robinson Law should be modified to read that the form of the curve is a constant *at a given stage of practice,* since the

form is a function of practice and Stroud (202, 203) has shown that the form is a function of the complexity of the material and of the learner.

Backward and Remote Associations. The questions of the existence of backward and of remote associations have remained unsettled since Ebbinghaus first raised them, but experimentation upon them has been relatively small. Recently, however, they have been attacked by Hunter (95), Dodge (55), Lumley (129, 130, 131), and Mitchell (163, 165). Hunter concludes from a review of the data that, in learning, the backward order of association is the usual one, although the direction of recall may be forward. With regard to remote association, Lumley and Mitchell have found with a variety of verbal materials that subjects tend to anticipate responses which would be correct later on in the series, thereby indicating the existence of remote forward association. Corresponding evidence for association in the backward direction does not appear in their data. On the basis of the facts of anticipatory reaction, Dodge concludes that the doctrine of direct bonds between specific stimulus and specific reaction is either an artifact or a limiting case. His paper contains many examples of remote associations.

These contributions to the problem have already provided valuable data which any theory of learning must take into account and have called more clearly in question than related work has done hitherto the doctrine that verbal learning is a matter of simple serial chaining.

The Influence of Temporal Variables. The problem of association as a function of the time-interval between the terms to be associated overlaps that of remote association, since valid remote associations imply formation across a time-gap.[12] It is, moreover, interwoven with the classical law of contiguity. The fact that experiments have shown rate of associative formation to be a function of time interval between presented terms leads Robinson (183) to interpret contiguity as a quantitative law expressing a continuous relation between time-interval and association. Guthrie (78) doubts the existence of any general law and suggests, further, that some coincidence of associated items may be necessary. Whether there is a single law may well be doubted, as Robinson would, but the absence of one leaves unaffected the validity of contiguity as a continuous function of the interval between presented terms. The recent experimental and theoretical

[12] See Wolfle (240, 241) and the earlier papers reviewed by Carr (29), Guthrie (78), and Robinson (183).

attacks upon this general problem have taken contiguity out of the armchair in which it has reclined with such dignity for years and have held it up to a less solemn but more honest scrutiny. It looks in this light much more able to defend itself against both the condescending humor and the indignant charges of its Gestaltist foes.[13]

Set and Incidental Learning. We have long known that speed of learning and amount of retention may be greatly influenced by intent to learn, learning with intent to retain, and by other forms of 'set.' Incidental learning, or learning without deliberate intent, is much less than learning under set or instruction to learn, but some incidental learning nevertheless occurs and its occurrence offers a problem to theories of learning. In an important paper on the problem Jenkins (**100**) has shown that incidental learning may be greatly affected by the self-instructions of the subjects and by occasional instructions derived from the experimental situation. His data imply that the difference between incidental and intentional learning may be much larger than has been supposed and suggest strongly the necessity for post-experimental reports in work on this and other problems.[14]

A Brief Summary of Other Problems. (1) *The Whole-Part Problem,* which was long discussed under the influence of the constancy attitude, has been more recently attacked by a search for the conditions which determine the relative status of whole and of part learning.[15] (2) The influence of *context,* so often tacitly assumed or openly neglected, has been studied in a variety of ways,[16] again with results which vary with experimental conditions but which show a considerable constancy of performance under variable context. (3) *The character of the material learned,* which has since the earliest studies of learning been recognized as a major variable, has been studied in new forms by Cason (**34**), Conrad and Jones (**40**),

[13] On studies of other temporal variables see Cason (**34**), Maslow (**136**), Ribsskog (**182**), and Seibert (**189**).

[14] See, also, Balken (**7**), Barr and Park (**10**), Briggs and Jordan (**21**), Haefner (**79**), Thisted and Remmers (**207**), Thorndike and Rock (**216**), and Whitely and Blankfort (**233**). Compton and Young (**39**) report, under the heading of 'set,' work which is closely related to both memory span and delayed reaction.

[15] For the evidence that no widely unqualified generalization can be made, see G. O. McGeoch (**138**). For experimental work under the relativity attitude, see Crafts (**42**), Davis and Meenes (**46**), G. O. McGeoch (**137, 139**), Seibert (**189**), Stroud and Ridgeway (**206**), and Winz (**237, 238**).

[16] Freeman (**71**), Maslow (**135**), Pessin (**171, 172**), Seibert (**189**), and Whitely and Blankfort (**233**).

Johanssen, Stirling and Levine (104), Karwoski (107), and Wilson (234). (4) Related closely to the foregoing problem is that of the influence of *the affective tone of the material.* Balken (7), Cason (32), and White and Ratliff (232) fail to find any major influence of affective tone except under certain special conditions.[17] (5) The problem of *motivation* has been investigated much more vigorously in the field of motor learning than in that of the learning of verbal materials. A paper by Leuba (118) bears upon the general problem. An experiment by Warden and Cohen (225) fails to find a clear advantage for special incentives under school room conditions, while one by Brown (22) finds such an advantage. The close relation of motivation to the law of effect should be noted, although the two are often discussed independently.

A number of important papers on conditions localized in the learner have appeared during the four years. (6) Work on *the relation of age to learning* has proceeded toward a search for the conditions which determine the relation. The character of the material and prior practice at study have been shown to be important variables,[18] and MA has been found to be one of the principal determiners of the CA learning relation.[19] (7) *Sex differences,* which had become a futile but respected problem, have been studied recently with the relativity attitude in a search for determining conditions, and it has been demonstrated that sex differences are a function of the interrelations of prior training and the character of the material learned.[20] (8) *Correlations between measures of intelligence and of learning* yield a considerable number of high coefficients. The amount of correlation is a function of the material learned and of other attendant conditions.[21] (9) *The influence of sensory mode of presentation* has been given unusually thorough study by Koch (112), who deals with a number of the conditions upon which the relation between modality and rate of learning depends.[22] (10) *Personality*

[17] *Cf.,* in this connection, the papers by Bunch and Wientge (25), Cason (31, 33), and Pyle (176). Cason's (32) monograph contains a review of the literature and a large amount of new experimental data.

[18] Brunswik, Goldscheider and Pilek (24), Conrad and Jones (40), Dietze and Jones (53), and Sorenson (195).

[19] Dietze (51) and Stroud and Maul (204). It is worthy of note that the CA-learning relation during maturity continues to receive attention (40, 51, 195).

[20] Boynton (20), Conrad and Jones (40), and Dietze (52).

[21] Bolton (14), Dietze (51), Fry (73), Stroud and Maul (204), Vértes (224), and Wilson (234, 235, 236).

[22] See, also, Kirk (109), Port (174), and Seibert (189).

traits have seemed to many investigators to be a condition of rate of learning but their influence has seldom been measured. The four papers [23] recently published on the problem open it to further experimental attack.

(11) *Inter-serial effects* appear inevitably in so many experiments upon verbal learning that they constitute one of the major conditions to be controlled in work upon other factors. They have been specifically studied by a few recent investigators [24] and offer an important problem for future study. (12) *Intra-serial effects* are at least equally inevitable and enter as a problem into any theory of organization. They have been given a fresh attack in five recent papers [25] on the general question of serial position and two publications on interference [26] have dealt with both inter- and intra-serial effects. (13) Anastasi's studies of a *memory factor* are models of careful analysis, and her intercorrelations, with the discussion of their determining conditions, are highly important.[27] (14) A number of *other problems* have received important investigation; among them are the influence of recitation,[28] vividness,[29] memory span and delayed reactions,[30] directed attention,[31] refractory phase,[32] racial differences,[33] and qualitative description.[34]

III. Experimental Work: Retention

Curves of Retention. The discrepancy between the results of Ebbinghaus and those of other investigators, particularly of Radossawljevitch, has often been referred to a difference in the

[23] See Stern's (**201**) theoretical discussion and the experimental papers by McGeoch and Whitely (**153**), McKinney (**156**), and Wylie (**243**).

[24] See the historical and theoretical discussion by Norem and Wiederaenders (**167**) and the experimental papers by McGeoch (**147**), Mitchell (**162**), and Wells (**228**).

[25] Crafts (**43**), Droba (**57**), Foucault (**68**), Krueger (**113**), and Mitchell (**164**).

[26] The monograph by Skaggs (**192**) deals with the major forms of inhibition and the paper by James (**99**) with the general problem of interference.

[27] Anastasi (**4, 5**). See, also, Arons (**6**).

[28] Seibert (**189**) and Skaggs, Grossman, Krueger and Krueger (**193**).

[29] Van Buskirk (**220**).

[30] Emerson (**64**), Hurlock and Newmark (**96**), and Skalet (**194**).

[31] Krueger (**113**).

[32] Luh (**127**).

[33] Louttit (**126**).

[34] Bowers (**18**) and Davis (**48**).

criterion of learning. Ebbinghaus is said to have used a criterion of one perfect trial while Radossawljevitch used two. Van Ormer and Dallenbach (222) enumerate some of the repetitions of this explanation, which is in error. Ebbinghaus' criterion was two correct repetitions.[35]

The experimental results on the curve have, during the last four years, extended our knowledge to new materials but have added little which is new in principle or which requires new theoretical interpretations.[36]

Repeated Reproduction. More theoretically interesting have been the studies of changes in remembered content with repeated reproduction and of the conditions of these changes. Bartlett's (11) experiments lead him to a theory of memory and to certain theories of importance for social psychology. The work of Hartgenbusch (83) and of Perkins (170) is of the same general kind, while Carmichael, Hogan and Walter (28) study the effect of language on the reproduction of visually perceived form.

Retroactive Inhibition. The influence of the inhibitory effects of interpolated activities has been the subject of an unusually large number of investigations,[37] which have extended our knowledge of the conditions affecting degree of inhibition and which have, as well, shown more clearly than hitherto the generality of the phenomenon. Especially important is the monograph by Van Ormer (221) in which he reports a repetition of the Jenkins and Dallenbach experiment on retention after intervals of sleep and of waking. His corroboration of the main findings of their experiment, when retention is measured

[35] For discussion of a parallel error in the naming of the Ebbinghaus curve, see Chou (38) and Bancels (8).

[36] Bassett (12), Cederstrom (37), Dietze and Jones (53), Ellis (63), Greene (77), Kennedy (108), Layton (115), Luh and Chiang (128), and Seibert (189).

[37] A list of the sub-problems will summarize the scope of the work: (1) affective tone (Bunch and Wientge, 25; Frank and Ludvigh, 69); (2) similarity (Dreis, 56; Johnson, 105; McGeoch, 142; McGeoch and McDonald, 151); (3) mental set (Lester, 117); (4) interpolated shock and emotion (Harden, 81; White, 231); (5) degree of interpolated learning (McGeoch, 143); (6) material learned (McGeoch and McKinney, 152); (7) time interval (McGeoch, 149); (8) temporal point of interpolation (McGeoch, 150); (9) grade level (Dreis, 56); (10) direction of interference (Maslow, 136); (11) suggestion and hypnosis (Hull, 93; Mitchell, 160, 161); (12) implications for theories of memory (Restorff, 180); (13) summary of literature (Skaggs, 192); and (14) importance as a determiner of forgetting (McGeoch, 144; Van Ormer, 221).

by saving instead of by recall, is significant factually, methodologically and theoretically.

Context and Retention. The recurrence at the time of recall of stimulating conditions similar to those present during learning seems to be one factor of which retention is a function and a change in these conditions seems to be one determiner of forgetting. Relatively little work has been devoted to this problem in the past, but during the years under review five papers[38] have contributed directly to it. We need to know much more about the influence of degree of similarity and of other aspects of the two temporally and categorically distinct stimulating conditions.

Recall of Interrupted Tasks. Zeigarnik's well known results upon this problem have aroused a considerable interest which is reflected in recent papers. Schlote (187) has verified the Zeigarnik phenomenon and has gone on to seek for its conditions. He believes it to be explicable in terms of the concept of determining tendency. From a study of tonus during completed and interrupted work Freeman (72) believes that the Zeigarnik phenomenon is explicable in such neural terms as competition, reinforcement and inhibition, rather than in terms of "psychic tension."[39]

The Influence of Set and Suggestion. This problem is probably closely related to the preceding one. Geyer (74) finds that a change in the expected time of recall is detrimental to retention, and in a number of studies the influence of waking suggestion and of hypnosis has been explored.[40]

Affective Tone and Retention is another question upon which recent experimentation has been unusually active.[41] It gathers significance from its relation to motivation, the law of effect, the Freudian theory of repression and theories of affective memory. The current work upon it is marked by a critical attitude toward methods of attack, by a search for new methods and by more careful

[38] Burri (26); Carmichael, Hogan and Walter (28); Hansen (80); Maslow (135); and Reed (179).

[39] See, also, Birenbaum (13), and Mahler (134).

[40] Hull's (93) book contains a great deal of important material upon hypnosis and retention. See, also, Huse (98), Mitchell (160), and Stalnaker and Riddle (198).

[41] The monograph by Cason (32) reviews the literature and reports a large amount of experimental data. See, also, Jersild (101), Jersild, Mackey and Jersild (102), Koch (111), Lynch (133), Meltzer (157, 158, 159), Stagner (196, 197), White and Ratliff (232), and Young (244). On the problem of affective memory, see Delacroix (50) and Dugas (60).

and critical interpretations of the results, as well as by an investigation of determining conditions.

Age and Retention. The papers upon this general problem fall chiefly into two classes: (1) those which deal directly with memorial performance as a function of age and (2) those which report the memories of individuals of varying ages for childhood experiences. The most unique piece of work belongs only partially to the first. Burtt (27) daily read aloud 20-line selections of Greek drama to a boy aged fifteen months at the beginning of the experiment. This continued, with a change of material every three months, until age three. At age eight and one-half the boy learned this material by a modified prompting method and at the same time learned other similar material *de novo*. The material which had been read aloud during infancy was learned with considerably greater ease than was the new material. The large percentages of saving, after so long an interval, from the presentation of material meaningless to the subject are interesting and of importance for theories both of learning and of retention. Three other valuable titles [42] in the first category deal with memory during the early years of life.

The method of the studies of the second kind has been to request reports of early childhood memories. Those of recent years show an improvement in technique and a greater appreciation of both the methodological and theoretical issues involved.[43]

A Brief Summary of Other Problems. (1) *Abnormalities of memory* have been reviewed in a volume by Dugas (62). (2) *The influence of the character of the material.* The older generalization that skilled acts are better retained than are verbal materials is called in question but the determining conditions in any given case remain to be discovered.[44] (3) Ewert (65) has measured eye-movements during reading and recall. (4) Various aspects of *methodology* have been usefully discussed.[45]

[42] Hurlock and Schwartz (97), Hetzer and Wislitzky (87), Stern (201). See, also, Stroud and Maul (204) on the relations between saving scores and age over a considerable age-range.

[43] Crook and Harden (45), Dudycha and Dudycha (58, 59), Jersild, Mackey and Jersild (102).

[44] Freeman and Abernethy (70), McGeoch (145). See Brown (23) and Hausen (85) for studies of other aspects of the relation between material and retention.

[45] Henning (86), McKinney (155), Pauli (169).

IV. Some General Tendencies

It has been possible to give only the briefest of summaries of major problems. An attempt has been made, however, to organize the titles in a way to make them more readily available for use. In addition it is interesting to inquire in what general ways, if in any, the publications of the last four years represent an advance over those of preceding periods.

The answer to this question will vary somewhat with the reviewer, but there are certain general characteristics which seem to the present reviewer significant. A greater concern for methodology, as such, has begun to appear and with it more careful experimental procedure in more experiments. The day is passing, when anyone whatever, regardless of training and acquaintance with the literature, thinks of doing experiments on memorizing. More experiments have been done systematically to extend our knowledge of the problem worked on rather than merely to do 'another experiment.' Theory has notably become both more precise and more general. The older search for absolutes, the quest for constants, is slowly giving way to a relativism which views phenomena as multiply conditioned variables to be studied in correlation with their conditions. New problems have been attacked or old ones better stated and there have been fewer reports on sterile problems.

Many things in the work of the period are open to adverse criticism. There has been work by questionable methods, work done with little systematic plan, work done in quest of constants and theorizing which seems futile, but these grow less. We know better than we did how to attack the dozens of new problems which sprout from every good piece of work already done.

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THE LAW OF EFFECT AS A PRINCIPLE OF LEARNING[1]

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The present review gives a summary of the more important studies, experimental and theoretical, that have been published on the law of effect. So general is the principle and so wide its possible application to experimental results that the reviewer is at once confronted with the difficulty of selection. On the one hand the whole problem is closely related to that of motivation and on the other hand to that of the relation between feeling tone and learning and retention. The field of motivation has recently been summarized by Diserens and Vaughn (18) and that of affective tone, learning and retention has been rather thoroughly treated by Cason (12) which has somewhat lightened the reviewer's task in that he is thus left those studies which bear more directly upon the law of effect as a principle of learning. Even so, there is necessarily some overlapping for which no apology need be offered.

I

The fact that the law of effect, as usually stated, represents the end result of an extended theoretical and experimental development has been shown by Cason (13) and by Stephens (51). Omitting this historical development in Thorndike's own thinking, we have from his pen in 1911 (55) the following statement of the law which provided contemporary psychologists with a point of departure for a number of theoretical and experimental papers: "*Of several responses made to the same situation, those which are accompanied or closely followed by satisfaction to the animal will, other things being equal, be more firmly connected with the situation, so that, when it recurs, they will be more likely to recur; those which are accompanied or closely followed by discomfort to the animal will, other things being equal, have their connections with that situation weakened, so that, when it recurs, they will be less likely to occur. The greater the satisfaction or discomfort, the greater the strengthening or weakening of*

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the bond." Thorndike in 1913 (**56**) gave another statement of the law in much the same words, but then emphasized the fact that the "strengthening effect of satisfyingness (or the weakening effect of annoyingness) upon a bond varies with the closeness of the connection between it and the bond." This same statement is repeated by Thorndike in 1932 (**59**) but here he changes his attitude concerning the relative values of satisfiers and annoyers. Up to this time, although he presented some data in 1931 (**58**) against the assumption, the two—satisfiers and annoyers—were supposed to have equal influence on learning. After the experimental findings reported there and in 1932 (**59**) this supposition appears no longer tenable; satisfiers "can be relied on to strengthen the connection" but annoyers have "no such uniform weakening effect." The degree of closeness between the satisfyingness and the bond in question is now held to be conditioned by his new principle of belongingness and the impression is left on the reader that rewards strengthen those bonds that precede them by 0–2 seconds and have very little effect on temporally more remote bonds. Another noticeable feature of Thorndike's recent writings (**57, 58, 59**) is that the experimental evidence he adduces in defense of the law of effect is drawn largely from experiments on the effect of knowledge of results. So much is this the case that one is led to wonder whether the law of effect is now anything more than the law of knowledge of results, as Rexroad (**43**) has said. The reviewer does not think this would be Thorndike's attitude. What we have to do, Thorndike would argue, is to set up some situation in which satisfiers or annoyers may operate and whether we do this by giving knowledge of results, electric shock, or any other device is a matter of indifference or experimental convenience; the effective agents are and remain affective in nature.

While some such principle as the law of effect had been mentioned by psychologists prior to Thorndike, as Hollingworth (**23**), Cason (**13**), and Stephens (**51**) have shown, Thorndike is to be credited with first giving a precise formulation of the law and with bringing it definitely within the domain of experimental psychology. Educational psychologists quickly grasped the principle and made extensive use of it; Gates (**19**), Jordan (**29**), and Sandiford (**47**), to mention typical instances. On the other hand, writers of general introductory texts in psychology have not been unanimous in their acceptance of the law, as will be indicated more fully later.

Thorndike's original interpretation of the law of effect has been subjected to severe criticism by a number of writers. These criti-

cisms may be grouped into four rather distinct groups: First, those which have been aimed at the implied or admitted retroactive working of the effect; second, those aimed at the philosophical implications of the law; third, those that deal with the question as to what the effective or distinctive conditions are that do cause learning according to the law of effect; fourth, those that are directed toward the generality or comprehensive usefulness of the law. Instances of these criticisms will be given in order.

Before beginning this phase of the review, however, perhaps another point should be emphasized, namely, that *there are two things involved in the law of effect.* This fact has been indicated by Carr (6), Thorndike (57), Tolman (61), and Dashiell (16). The one of these is that progress in learning is determined by the effects or consequences of acts already performed by the individual in response to the situation confronting him. The second concerns the determination of the particular results, effects or consequences that cause this progress. Thorndike's original statement as given above does not clearly embody both of these items and, as it is there stated, should be interpreted as being a formulation of what he considers the effective conditions to be, *viz.*, that they are affective in character. It is as though he assumed the general fact that progress in learning is determined by the consequences of earlier acts and was interested in pointing out that the effective consequences were the satisfyingness and dissatisfyingness attendant upon these acts. In Thorndike's more recent writings (57, 58) this point is explicitly made and hence supports the belief that even in his first formulation he distinguished between these two items. If this reasoning is correct there would seem to be a more general principle which might best be called the law of effect [2] to cover the first comprehensive approximation and then a number of subsidiary laws which specify the particular conditions that are effective. That the law of effect in this general sense is accepted by psychologists is amply shown by the numerous studies cited by Diserens and Vaughn (18). Again, if this suggestion is valid it will be seen that some of the criticisms to be considered are

[2] Stephens (51) implicitly makes the suggestion that this general law be referred to as the law of subsequent stimulation. However, the law of effect seems to the writer to be somewhat more precise in that it indicates more particularly what subsequent stimulation is intended than does the phrase 'law of subsequent stimulation.' The law of effect does have an unfortunate historical meaning since it is taken most widely to refer to affective consequences as being the fixating factors, but it would seem that such a change as here suggested would not seriously embarrass psychologists.

germane only to some one or other of the special laws and not to the general law.

II

To return to the consideration of the various types of criticism that have been made of the law of effect, first treatment will be accorded those that deal with the retroactive working of the effect. This criticism has been, of all the others, the most frequently mentioned and is usually stated as follows: How can an effect or a consequence of an act have any influence, detrimental or beneficial, on the retention and repetition of the act which preceded and produced it? This criticism is offered by Dashiell (**16**), Peterson (**39, 40**), Rexroad (**43**), Carr (**6**), Snoddy (**48**), and quite possibly by others. Thorndike, however, apparently defends the statement (**57, 59**) although he at times seems to argue (**58, 59**) that the effect works on the connection at the time and hence that there need be no retroactive effect.

The significance of the controversy concerning the problem of the backward working of the effect must be considered in terms of the general learning theory (as held by Thorndike [**55**] and by Watson [**67**], for example) which forms the framework within which the law of effect was placed. The general theory seems to have been that the subject's performance was made up of numerous discrete acts and that progress in learning came as a result of the selection and fixation of certain of these acts and the elimination of others. Thus the general picture might be presented as follows: During the first trial (on a maze, for example, although any other illustration would serve as well) the subject performs a series of acts A B C D E F G H I before performing J which leads into the goal compartment. (Peterson [**41**] gives a succinct statement of such a theory.) Of these acts certain ones are errors, C D G I, let us say, in that they lead into the blinds. Now, after a series of trials, or when learning is complete, the subject performs this series—A B E F H J. These particular acts are the same that he performed on his first trial. The correct series of movements finally fixated is thus seen to be taken from those performed during each of the preceding trials.

If this be the general theory back of the law of effect as it has usually been stated, then it seems that he who holds to this theory cannot be excused from treating of what, on the surface at least, appears to involve the backward working of the effect. Either this phenomenon must be assumed, must be plausibly or adequately

accounted for, or be shown not to exist. I think it should be pointed out, also, that those who have urged this criticism most strongly do so on logical grounds only. Nowhere do they adduce any evidence of an experimental sort that such a phenomenon cannot happen. It seems to the writer that a part of the difficulty encountered in connection with this interpretation, *i.e.*, that the effect must work backward to achieve its results, is chargeable to the English language and in part to the limitations of observation. The sequence of things *seems* or *appears* to be—stimulating situation, reaction or response, effect or consequence. *Apparently* the effect occurs *after* the act has been performed, but there are many cases in which the effect may occur while the act is being performed, as asserted by Carr (**6**) and Reed (**42**), and where this does not seem to be the case there is no difficulty in assuming that there are immediately-persisting intra-organic conditions existing as a result of the act (these conditions might be conceived of as a part of the act if one cared to) and that the effect occurs while these are present. In both cases the resulting strengthening or weakening of the linkage between the situation and the response occurs without involving the concept of the backward working of the effect. This is apparently the interpretation recently indorsed by Thorndike (**58, 59**).

Some writers have rallied to the support of the proposition that the retroactive working of the effect is not impossible and have devised hypotheses by means of which such a phenomenon can be explained. Most notable is Troland (**63**) who develops the principle of retroflex action, stating the principle as follows: "*Nociception is accompanied by a decreasing of the conductances of operating cortical adjustors; whereas beneception is accompanied by an increasing of the conductance of operating cortical adjustors.*" According to this principle, the act performed by the subject may result in either favorable or unfavorable consequences with reference to the continuance of that act. If the consequences are favorable this is 'reported' to the cortex by means of afferent fibers and the excitations thus initiated either enhance, or if unfavorable reduce, the tendency in question. This is achieved by an increase or decrease in the conductance of the cortical processes involved.

Stephens (**51, 52**) has contributed another suggestion as to a possible physiological explanation of the manner in which such retroactive effect may be produced. His suggestion involves the conception that the subsequent stimuli arouse nervous impulses which, through a hypothetical system of nervous mechanisms directly affect

the bond or bonds that aroused the preceding act. This conception differs from Troland's in that no assumption is made concerning the facilitation or inhibition of nervous discharges. Rather the return impulses from this conceptual system act directly on the bonds involved to strengthen or weaken them. Just how a third neurone can affect or influence the bond between two other neurones presents a formidable problem and is frankly admitted as such by Stephens. Still, as he points out, " The ascription of such powers as the power of influencing other connections seems not a very great additional burden to be placed upon these overworked hypotheses carriers " and this added bit should not frighten theorists. Stephens has incorporated these suggestions into a mechanical model which he argues embodies the features of the law of effect (**50**).

Haggerty (**20**) has suggested another hypothesis which would explain how the concept of retroaction can be avoided. This is in effect similar to the one first suggested, which was based on the assumption of the persistence of some intraorganic condition upon which the effect may work, but assumes that " one physiological state overlaps and reaches beyond its immediately neighboring states." This conception he embodied in the *law of irradiation* which reads as follows: *"A physiological state is not self-contained but it tends to flow over and mingle with other physiological states and extends to states temporarily removed from it."* This conception has the merit, or perhaps the demerit, of suggesting rather specifically where the immediately persisting conditions supposed by the first explanation are to be found.

It should be noted, in leaving this part of our review that the type of criticism we are considering is applicable to the general law described above as well as to any of the special laws in so far as these are held to apply to the type of learning theory outlined above.

It seems to the writer, however, that a truer conception of learning would be one that did not assume that the subject performed a series of more or less discrete acts and that progress in learning is achieved by a process of selecting and binding together certain ones of these acts to form the final correct performance. Rather a new and different performance is executed on each successive trial and progress is made by achieving a better and more adequate mode of adjusting to the situation. This type of theory has received some attention by Peterson (**39**), Lashley (**33**), Wiltbank (**72**), Wheeler (**69**), and is perhaps the type of conception Cason (**14**) is presenting. Evidence for such a conception is found in a study of the

tracings made in mirror drawing, Snoddy (48); Adams (1) presents evidence in harmony with this theory in his study of the way in which cats adjust to the problem box; similar findings are reported for guinea pigs on the problem box by Muenzinger (36); probably the data on human subjects in similar situations would offer additional experimental evidence of the validity of this theory. This theory seems more in harmony with the experimental facts and also lessens the difficulty of interpreting the *modus operandi* of the effects or consequences of the performance. This follows since we are not now compelled to indicate how the effect can act to select or fixate already performed reactions but rather how it works for the production of better successive performances. The most useful assumption for this latter problem seems to be that there is left a complex 'disposition,' made up of the residua of the performance plus the results or effects of that performance, which 'disposition' operates in the production of better performances when the situation which the individual is required to learn is next presented.

III

The second type of criticism is aimed at the philosophical or metaphysical foundations of the law. This criticism is put forward by Peterson (40) who points out that the law implies an enigmatic dualism; by Cason (11) when he argues that according to the theory pleasure and pain must be looked upon as substantive faculties which can influence behavior. Carr (6) also criticizes the original formulation of the law by Thorndike on the same grounds, arguing that pleasure and displeasure are not observable items of experience but belong in the category of judgment. Troland (63) mentions this criticism and trains his guns on those who balk at the assumption that mental events can influence behavior simply because we cannot see how it can be done. He then proceeds to develop his theory of retroflex action, assuring the antagonists that if they find the pleasure-pain-effect of behavior unpalatable, they can adopt his physiological interpretation. Kuo (32) reëmphasizes the general criticism and interprets the suggestions of Hobhouse (22) and Holmes (25) as being efforts at avoiding the interactionistic hypothesis involved in the Thorndikian statement. Their attempts, however, according to Kuo, amount to naught. They simply substitute 'confirmation' and 'inhibition,' 'congruent' and 'incongruent' instead of 'pleasure' and 'pain,' and we are still left without an adequate explanation. Kuo also points out, as have others, *e.g.*, Carr (9)

and Watson (67), that all successful acts are not pleasant and likewise that all unsuccessful acts do not entail unpleasant consequences. Swift (53) argues that Carr's theory of sensory consequences or sensory intensity theory is the same as Thorndike's except for the differences in terminology.

Thorndike attempted (55) and again in (59) to give an objective definition of satisfyingness and dissatisfyingness in terms of the behavior of the organism. Satisfying states of affairs were described as being those which the animal does nothing to avoid, often doing such things as attain and preserve them, while annoying states of affairs were those which the animal avoids or abandons. This attempt, however, as Carr (10) showed, resulted in a circular type of argument only, which was not and is not satisfactory. To refer the fixating factors to the life-processes of the neurones, as Thorndike did in 1911 (55) and in 1931 (58), is an interesting speculation, which may, of course, be true, but such an hypothesis is at once removed from the possibility of experimental test, McGeoch (34) and Wilson (70).

Two comments may be made with reference to the type of criticism under consideration. First, this criticism is applicable only to one of the special laws, *i.e.*, to that in which the fixating factors are held to be affective in nature. Second, this criticism takes its origin from another philosophical position so that what we have here is an argument which in reality involves the validity of one philosophical position as opposed to another, a problem which does not permit of ready solution.

IV

The third type of criticism turns upon the question of the effective or distinctive conditions that do cause learning according to the general statement of the law of effect. Thorndike's original statement, be it remembered, argued that they were affective in nature. Carr (6) holds that the effective factors are the sensory consequences that occur while the act is in progress. He attempted in 1914 (9) an indication of the nature of some of the sensory consequences that do cause the selection of the correct or successful act or series of acts in the problem box, maze, and the inhibition of an instinctive or habitual performance. In all cases the general principle adopted is stated as follows: "Acts are selected or eliminated according to whether the sensory consequences tend to facilitate and intensify them on the one hand, or to disrupt and suppress them on the other."

Cason (11) argues that the fixating factors cannot be affective in nature, as has already been indicated, but offers no other factors in substitution. Hollingworth (24) argues against the affective factors being the effective ones. He argues that the difficulty has been that the affective factors occur frequently as correlative with the effective factors that are probably objective in nature: that is, that the original Thorndikian statement should be called the 'law of affect.' Snoddy (48) argues against the factors of pleasantness and unpleasantness and seeks a physiological explanation of the learning process, as also does Swift (53). Peterson (39) offers his theory of completeness of response as a substitute for the affective factors suggested by Thorndike. Kuo (32) argues against the affective factors but certainly cannot be said to deny the efficacy of the law of effect as can be seen from this statement made in connection with his own explanation: "But whether or not they are successful acts depends upon the consequences they bring about with reference to the determining tendency." That is, Kuo assumes that the important thing that acts as the selective agency is the drive or determining tendency that is operative and the relationship between this drive and the consequences of the act performed and it seems implied that these consequences are sensory in character. Hsaio (27) develops somewhat the same type of explanation after subjecting the Thorndikian factors of satisfaction and dissatisfaction to a rigorous criticism. Needs, for Hsaio, are the fundamental conception in explaining learning. These needs irradiate neurones(!) that are directly concerned with them. The efforts at escape are simply irradiations of the need. The correct movement is the last irradiation and it satisfies the need, thus relieving the tensions that accompanied it.

Disregarding the argument as to whether or not the effective factors are or are not affective in nature, a number of papers have shown that knowledge of results does facilitate learning. It may be that when such knowledge is withheld some progress is made in learning (Carr, 6), but no serious doubt has so far been expressed that, when introduced, it does markedly increase the amount and rate of learning.[3] Evidence tending to establish this point is furnished by Thorndike (57, 58, 59), Arps (2), Judd (30, 31), Panlasgui and Knight (38), Spencer (49), Trowbridge and Cason (64), Deputy (17), Ross (46), Chapman and Feder (15), Mead (37), Book and Norvell (4), and Brown (5). Not all of the contributors

[3] During the past two years the writer has collected, in several experiments, some contrary evidence which will be published later.

agree as to the intrinsic nature of the mechanism by means of which information does lead to more rapid progress, although all agree on the experimental facts.

The beneficial effect of rewards and punishment is amply attested by the numerous studies reviewed by Diserens and Vaughn (**18**), Tolman (**61**), and others. Tolman, Hall and Bretnall (**62**) have recently contributed an experiment on a special type of maze in which information as to the correctness of the act performed was given by means of a bell or a bell combined with shock. According to their results, if the bell and shock are given on the correct movement no more effect is produced than by the bell alone. This leads them to the conclusion that the law of effect has been disproven and they substitute for it three other laws which will be mentioned below.

It is obvious that we are here dealing with suggestions toward the formulation of the special laws described earlier. Thus the literature contains the following suggestions: Law of affect (Hollingworth's [**24**] interpretation of the way in which Thorndike's law should be named); law of pleasant and unpleasant affects, and the law of knowledge of results (Rexroad's [**43**] interpretations of Thorndike's formulations); law of sensory consequences (Carr **6**); the three suggested by Tolman, Hall and Bretnall (**62**), the law of motivation, the law of emphasis, and the law of disruption. The last group of three laws are the only ones whose import has not been made clear by the preceding discussion. These are described by their authors as follows:

"By a Law of Motivation we would mean the assumption that the ultimate reason for learning seems to be the obtaining of final successes or of final failures. (And, insofar as the avoidance of shocks is in and of itself a potent goal, then the introduction of shock to the wrong responses will favor learning.)

"By a Law of Emphasis we would mean the assumption that in a trial and error situation learning consists in building up little patterns or gestalts in which the correct response is set over against the incorrect one or ones and the further assumption that an accent or emphasis (such as bell or shock) upon the correct responses will tend to favor learning whereas an accent or emphasis upon the wrong responses will tend to hinder learning.

"By a Law of Disruption we would mean the assumption that any relatively violent negative emotional stimulus (such as an electric shock) coming in immediate sequence upon either the right or the wrong response will, as such, tend to disrupt learning."

The objection is immediately granted that these special laws are all hypotheses, the point is that they are all offered as being fruitful attempts at a better analysis of the general law of effect or at least they can satisfactorily be so considered.

With each of these suggestions experimental tests must be undertaken to determine the various conditions that influence their working, the precise mode of their operation, etc., before any one can be considered as a useful principle of learning. Some of the work has already been begun. Thus Troland (63) through his principle of retroflex action, Stephens (51, 52), Haggerty (20), Thorndike (58, 59) have all contributed theories which attempt to explain the mechanisms by means of which the consequences achieve their effects. Kuo (32), Hsaio (27), Hollingworth (24), Barton (3), Book and Norvell (4) all argue that the important thing is the relationship between the motivating conditions, the drives or wishes of the organism, and the effect that follows an act which determines the fixating or eliminating power of the effect. Hollingworth earlier (23) suggested that the effect produced a change in the stimulus presented and hence produced selection or elimination. Dashiell (16) makes use of the principle of conditioning in an effort to account for fixation and elimination according to the law of effect. Trowbridge and Cason (64) argue that the use of ideas and ideational representation of the preceding act and its consequences is the essential feature of the mechanism involved in learning when knowledge of results is given.

Another phase of the investigation necessary for a complete understanding of the relation of effects, of whatever nature, to learning (and one that is made peculiarly important due to Thorndike's [59] treatment of the matter) is handled by those who have studied the nature of the temporal relations that must exist between the effect or consequences of an act and the latter's fixation or elimination. Watson (68) showed that a delay of thirty seconds in getting to the food in the problem box situation did not make for slower learning. This result he interpreted as being adverse to the law of effect. Warden and Haas (66), using the Warden maze pattern with rats, found that a delay of five minutes did not interfere with learning. A delay of one minute seemed to have a deleterious effect which was considered a chance effect only. On the other hand Roberts (44), using a problem box set-up in which the door to the food box was opened by the experimenter after the selected periods of delay had

intervened between the animal's touching a pendulum hanging in the problem box, found that delay periods of five, ten, and thirty seconds were detrimental and the longer the delay the greater was the detrimental effect. Likewise Hamilton (21), using the obstruction method and a maze learning situation, found intervals of fifteen, thirty and sixty seconds delay detrimental, and equally so, and that a delay of three minutes produced a further deleterious effect in the obstruction apparatus; delays, of one, three, five, and seven minutes in the maze learning situation were detrimental and about equally so. Warden and Diamond (65) present tentative results on a simple Y or T maze that indicate for delays of zero, four, eight, twelve, and twenty seconds in the administration of punishment (electric shock) a marked decrease in the speed of learning and the decrease is greater with increased delay.

These results appear somewhat contradictory until note is made of the different conditions under which the various experiments were conducted. Watson, Warden and Haas delayed their animals in the food compartment. That is, the animals were allowed to enter the food compartment but the food was covered, thus prohibiting eating until the delay period had elapsed. The remaining experiments, with the exception of Warden and Diamond, employed the delay intervals while the animal was outside the food compartment. In Roberts' work the animal was simply kept in the problem box after he had touched the pendulum, Hamilton delayed the animal in a maze compartment adjoining the food compartment. In the case of the two former studies, then, it could easily be argued that getting close to the food, being more directly stimulated by its odor, etc., furnished a closely integrated part of actually feeding, as Roberts and Hamilton point out. In the second group of two experiments this direct stimulation was not possible due to the controls introduced; and hence the difference in the results. (The reader is referred to Thorndike's [59] searching analysis of the results of these and the related studies.) In the case of Warden and Diamond the situation was necessarily somewhat different. After the animals had been trained to take the right hand turn, further training requiring them to take the left hand turn was instituted. In this case when the animal took the wrong turn, *i.e.*, the right hand turn, the punishment (shock) was administered at the selected delay periods. And this difference in technique introduces a further difficulty for the animals.

V

A fourth rather infrequently mentioned criticism is aimed at the assumed generality of the law. Tolman (**60, 61**) and Wheeler (**69**) both express or imply this criticism when they intimate that learning takes place when the law of effect is not operative. So far as the reviewer knows no one has ever argued either that there is no learning without the presence of the operation of the law of effect nor that learning in all its phases must be accounted for solely on the basis of the law of effect. In his original presentation Thorndike (**55**) held for two laws and also showed that the law of effect was conditioned by several other principles. Writers since have always included a number of different principles to account for learning. Thorndike did, however, believe that the law of effect was the most important of all principles. One reads his latest account (**59**) with a growing conviction that the usefulness of the law, in the light of his strictures placed upon it, may well be questioned in the case of more complex learning at least.

VI

Turning to a brief consideration of the place the law of effect is assigned in learning theory we discover several interesting facts. In harmony with the lack of unanimity concerning the effective conditions operative under the law of effect there is a corresponding divergence of opinion as to where the law belongs. An examination of current writing shows that some psychologists accord it a place in accounting for the selection and elimination of acts, Carr (**6, 8**), Dashiell (**16**). Others discard it altogether. For example, Woodworth (**73**) does not mention the law in his index nor in the chapter devoted to learning. One reads of a ' check-up ' being offered by the environmental conditions and this check-up as accounting for the elimination or retention of acts, but this is not subsumed under the general principle of the law of effect. Hunter (**28**) quotes Thorndike's law but leaves it without serious comment. Wilson (**70**) and Winsor (**71**) give treatments of learning which are typical of conditioned reflex theories and take no account of the law of effect. Symonds (**54**) draws up a set of laws of learning couched in the language of conditioning but the law of effect is absent from this list. Holt (**26**) believes he can, with the aid of the concept of the reflex-circle, Bok's law of stimulogeneous fibrillation, Kappers' law of neurobiotaxis and the principle of conditioning, account for all

learning without recourse to the law of effect. Wheeler (**69**) also discards the law, partly because of his systematic position which makes all the laws of learning, as conventionally stated, obnoxious, and partly because he disagrees with Thorndike's interpretation of the law, that is, he is primarily arguing against the law of *affect* and not the law of *effect,* since it is quite obvious that the general facts of the influence of consequences of earlier performances are accepted by him. Hollingworth (**23**) considers the law of effect a crude, rule-of-thumb method, satisfactory for rough work but not at all showing the preciseness of a good law (words that sound strangely like those used by Thorndike when he first proposed the law in 1911.) Rexroad (**43**) accords the law a secondary place in accounting for elimination. Tolman (**60, 61**), and Tolman *et al.*(**62**) discard the law as usually stated, here again they discard a law of *affect,* not one of *effect.*

Now if we hold to the traditional theoretical framework in which the law of effect was originally placed we note that the law of effect has come to refer to selection and elimination. Hence in so far as these two processes are basic, fundamental or primary features of learning then the law of effect, as reinterpreted in the earlier sections of this paper, must be considered a primary law of learning. If we hold to the second theory of learning outlined above to the effect that learning consists in the making of progressively better performances, the law of effect is still one of the primary laws if it is a chief factor in making for these better performances. The law needs supplementation as Carr (**8**) has indicated. It needs to be supplemented in three ways: First, it needs to be supplemented by laws governing other phases of the learning process. Second, we need a further knowledge of the effective factors, or special laws, which do make for progress in learning under the general law. A third type of supplementation is needed in connection with the precise mode in which the law operates. As for the first type of supplementation mentioned above it might be suggested that laws covering the *rate* of learning are needed. This point is raised by Carr (**8**) who holds that the law of exercise, when adequately modified, serves to account for this phase of learning. But is this law sufficient? If a law is a general statement of what occurs under certain given conditions then is there not sufficient evidence at hand to indicate that a number of other laws are justified? What of the factor of the 'will' to learn? Of distributed practice? Of active vs. passive attitude? Of guidance?, etc. Tolman's book (**61**) is instructive along this line. We have already indicated the types of special laws that are present in

the literature and contending for recognition. It is not implied that all of the suggestions given in connection with either problem are coördinate or free from overlap. The point is that these suggestions are to be found and work should be undertaken to test the validity and independence of each.

We have also noted above the various suggestions made toward an understanding of the *modus operandi* of the law of effect. Some of these suggestions are more consonant with the traditional theory than with the modification suggested but in so far as they are experimentally susceptible to attack, this attack should be made as vigorously as possible.

Another interesting angle to the discussion centering around the law of effect is given by the work of Carr (7) and Robinson (45). Both writers treat of the laws that operate in fixation and yet neither one mentions the law of effect, as has been noted, in the case of the latter, by McGeoch (35). This is the more remarkable in the case of Carr, who explicitly says that whether we speak of the laws of learning, of conditioning, or of association is a matter of terminology. In view of the amount of current writing on the law of effect it would seem that something might be done to bridge the gap thus apparently left between those principles utilized in accounting for learning and those utilized in accounting for the sequence of psychological processes. Are the principles different, the same, or in part the same and in part different? In which case in how far are they the same and in how far are they different? Reed (42) is the only writer, so far as the reviewer was able to discover, who has made an effort to fill in this gap and it seems, in the light of current discussion, that this matter stands in need of further systematic treatment.

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THORNDIKE'S THEORY OF LEARNING AS GESTALT PSYCHOLOGY

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I. Introduction

We believe that the future historian of science will discover only two basic viewpoints among the many psychologies of to-day, atomism and configurationism. Logical analysis of contemporary psychological theory points to the conclusion that association psychology, behaviorism, the introspective psychology of Titchener and Wundt and the purposive psychology of McDougall and others are essentially atomistic theories.[1] Opposed to all of them with a basically different methodology stands the Gestalt theory of Wertheimer, Koehler, Koffka, Lewin.[2] If we are right in assuming only two basic viewpoints it becomes important to attempt to shed light on the outcome of the struggle between them. In the field of perception the battle seems almost to be decided in favor of the configurational hypothesis.[3] In the field of learning the struggle between Gestalt theory and the atomistic hypothesis is particularly tense. However, as experimental data accumulate we believe that here too the victory will ultimately go to the adherents of Gestalt theory. We believe that in his latest writings Thorndike, himself long one of the staunchest supporters of psychological atomism, self-styled a connectionist, has unwittingly gone over to the other side. Our specific aim in writing this paper is to show that Thorndike's theory of learning could be successfully rewritten in terms of Gestalt psychology without serious modification

[1] Even vitalism we believe is generally conceded to-day to be nothing but a mechanistic atomism plus an unverifiable and hence scientifically worthless vitalistic force.

[2] W. Koehler, Gestalt Psychology, New York, Liveright, 1929. R. H. Wheeler, The Laws of Human Nature, New York, Appleton, 1932. M. Scheerer, Die Lehre von der Gestalt, Berlin and Leipzig, Walter de Gruyter, 1931.

[3] So at least Thorndike himself seems to imply.

of the tenets of either. Our hope in writing it is that such a demonstration will further a little our certain journey from the psychologies of to-day to the psychology of to-morrow.

II. Thorndike's Old and New Theory of Learning

In Thorndike's classical theory of learning all the phenomena of learning, from the modified avoidance reaction of paramecium to the general relativity theory of Einstein are explained by the three laws, (1) the law of readiness, (2) the law of exercise, (3) the law of effect.[4] Concerning the finality of these laws we let Thorndike speak for himself. "One form of misunderstanding these laws consists in supposing the necessity of additional factors." The wide experimental and theoretical criticism that the above laws have received is already familiar to most students of psychology to-day. But despite the criticism the laws remain the corner stone of the vast structure of American educational practice and theory.

In his recent book, "Human Learning,"[5] Thorndike casts aside his dictum concerning the finality of his laws and presents a decidedly modified theory, which we briefly outline:

(1) The law of readiness receives practically no consideration at all in the new book. (There are scattered references to the "principle" of readiness on pp. 103, 122, 127.)

(2) The law of exercise, or at least its sub law, the law of use, is almost completely abandoned. Mere repetition is declared to be of no avail.

"Repetition of a connection in the sense of mere sequence of two things in time has then very, very little power, perhaps none, as a cause of learning" (p. 28). This is shown in a series of rather ingenious group experiments on humans in the first and second chapters of Thorndike's book. That mere repetition could not carry the whole burden of animal learning has been clear to Thorndike for some time.[6] He makes, however, no reference to the broad literature on this problem. While the omission of the many well-known papers on animal learning available in English is perhaps to be understood, the present writers consider it a bad oversight on his part not to have

[4] E. L. Thorndike, Educational Psychology, Vol. 2, pp. 1 ff., New York, Teachers College, 1923.

[5] E. L. Thorndike, Human Learning, New York, Century Co., 1931. Our criticism is chiefly concerned with this book. Unless otherwise specifically stated all future page references refer to it.

[6] In his "Animal Intelligence," New York, Macmillan, 1911.

mentioned the work of Lewin on humans.[7] Lewin's experiments, performed in a more systematic theoretical setting than Thorndike's, gave identical results and forced Lewin to identical conclusions. Incidentally they were performed some ten years ago.

(3) The law of effect also receives considerable modification. "Satisfyers" retain their potency in the new theory, "annoyers" have limited power. Important as it is for the theory of learning, however, we will not attempt to deal with Thorndike's theory of motivation in a paper of this length.

Not only has the validity of the old laws been modified, but we find five distinctly new concepts postulated as necessary to explain human learning. They are (a) belonging, (b) identifiability, (c) availability, (d) trial, (e) system.[8] It is our contention that these conditioning factors, which in Thorndike's presentation are so many *ad hoc* hypotheses, functioning somewhat as the psyches of the vitalist, are logically prior to his other laws. We further believe ourserves able to show that these factors are the properties of organized wholes, in fact, the basic postulates of Gestalt psychology. As we criticize each one we will give at least one reference to papers by Gestalt psychologists in which the property under consideration was previously investigated in terms of Gestalt psychology.

III. Thorndike's New Postulates Criticized

A. *Belonging.* The chief new factor that conditions learning according to Thorndike is "belonging." Thorndike describes it as follows: "a sequence carrying with it a sense that the second thing belongs to the first or such a sequence plus the *sense of relatedness* or *belonging*" (p. 18, italics ours). Belonging, to Thorndike, is obviously not given in the stimulus as physically defined. It is therefore either something that accrues to two independent psychological events, or it must be a primary property of certain events. It seems to us that Thorndike is not clear on this point. But, if it is something accruing, through experience or anything else, it need not have been handled as a new postulate but could be treated by the old laws.[9]

[7] K. Lewin. *Z. f. Psychol.*, 1917, 77, 212–247; *Psychol. Forsch.*, 1922, 1, 191–302; *Psychol. Forsch.*, 1922, 2, 65–140.

[8] Perhaps Thorndike means to deduce (d) and (e) of the above list from other principles. Whether or not he does is really indifferent to our argument.

[9] Certain of his experiments could be severely criticized by other atomistic mechanists. It is by no means claimed even in Gestalt psychology that experience is of no influence. The belongingness between "Alfred" and "Duke"

Since Thorndike does not so treat it, he must admit it as a primary property of certain psychological events. Then our argument is simple. "Belonging" or sense of relationship implies organization (*i.e.*, Struktur, Gestalt), a property of wholes and only of wholes. Wholes cannot be treated as additively formed sums of parts. "Belonging" cannot be inherent in the parts as such. Thorndike and the Gestalt psychologists are talking about the same sort of thing.

The chief tenet of Gestalt psychology is simply this: *There are events in nature where there is organization and this organization is primary, so that the properties of wholes can not be understood as a resultant of the activities or properties of the individual parts.* This is *not* mysticism, it is *not* vitalism, it is *not* mentalism. It is simply a proposition about experience, possessing, until it is refuted, equal probability and equal epistemological validity with the opposite statement that the events of nature are composed of individual independent parts. This has been difficult of comprehension to many people because most students of science have had the atomistic approach drilled into them without any questioning of the epistemological validity of the atomistic assumption that wholes are made up of the summation of their parts.[10] The Gestalt psychologists in their investigations came upon certain psychological events that could not be explained by treating these events as additively formed sums of parts. In most cases, since they too had received the regular atomistic training, they tried at first to do this. But it was impossible. Thorndike has apparently recently come upon events of this nature.

It is enlightening to read Wertheimer's 1912 paper on apparent movement carefully to see what led him to set up the Gestalt hypothesis.[11] Strange though it may seem to readers without first-hand knowledge of this paper, the fact is that Wertheimer discovered nothing.[12] But, of far greater importance to the history of psychology, he thought through to their logical consequences the several existing atomistic theories regarding apparent movement and then

in "Alfred Duke" (p. 21), for instance, is very likely due to custom. R. H. Wheeler and F. T. Perkins, The Principles of Mental Development, New York, Crowell, 1932, p. 362, also question Thorndike's logic here.

[10] Wertheimer's paper, "Über Gestalttheorie," Erlangen, Philosophischer Verlag, 1925, contains very pertinent material on what Gestalt psychology is and what it is trying to do. Particularly enlightening are pp. 1–7. *Cf.*, also, R. H. Wheeler, S. H. Bartley, and F. T. Perkins, *Psychol. Review*, 1931, **38**, particularly 110–118.

[11] M. Wertheimer, *Zsch. f. Psychol.*, 1912, **61**, 161–265.

[12] We except his discovery of pure phi, *i.e.*, perception of movement without perception of moving objects.

he subjected these hypotheses to a series of brilliant critical experiments. He found that no existing theory could by the summation of independent local events explain apparent movement. He found out further that the likelihood of any further theory doing so was extremely slight. Hence he was *forced* to postulate the existence in nature of at least one psychological event in which the structure or organization or Gestalt is prior to the parts. After finding one such event he and his colleagues naturally enough began to look for others. They found many. Such in skeleton is the history of Gestalt psychology.[13]

In learning, Koehler found "belonging" an essential factor for learning in apes; Lewin found it equally essential for humans. Numerous other investigators have since made use of it.

"Belonging" then refers to the fact that certain psychological events exist as structures (Gestalten, configurations), irrespective of experience and prior to it. Learning occurs easily (i.e., without repetition) when the subject experiences such an event. If we have made ourselves clear concerning "belonging" we may deal more briefly with Thorndike's other factors.

B. *Identifiability* refers to "the qualities in a situation which make it easy to connect something with it, and the qualities in a response which make it easy to connect it with something" (p. 82).

In the Gestalt psychology it has long been recognized that certain structures are very stable and can be broken down only under certain conditions. Others are less stable. Considerable research has already been devoted to this property of Gestalten. Let us consider here only the work of K. Gottschaldt.[14] Gottschaldt demonstrated about eight years ago that the recognition or non-recognition of the simplest and most commonly experienced geometrical figures was conditioned by the whole in which these figures were imbedded. Furthermore, he showed that the stability of certain structures could not be modified, no matter how great the experience. It is very instructive to compare Gottschaldt's experiments with those of Thorndike (pp. 82–89)

[13] It is interesting to compare Wertheimer's experiments with the Michelson-Morley experiment. They seem to us to be methodologically equivalent. No one ever accused Michelson and Morley of mysticism but Einstein has been accused of mysticism and worse. Wertheimer in this case not only performed the critical experiments but made the necessary change in theory.

[14] K. Gottschaldt, *Psychol. Forsch.*, 1926, **8**, 261–317, and 1929, **12**, 1–87.

concerning "identifiability." Such a comparison convinces us that Gottschaldt and Thorndike were investigating the same thing.

Under certain conditions the relation of "belongingness" between two elements appears where it was previously lacking (Thorndike's nomenclature), or new structures emerge or are differentiated from existing structures (Gestalt nomenclature). This we call learning. *"Identifiability" refers to the fact that since structures vary in their stability, parts in certain structures are more readily differentiated into parts of new wholes than in others. Learning occurs easily when the task set for the subject is to "form a connection" between parts of less stable structures.*

C. *Availability.* "Consider now the principle of availability or get-at-ableness of the response, which is that, other things being equal, connections are easy to form in proportion as the response is available, summonable, such that the person can have it or make it at will" (p. 89). So far as we can see no really new principle is here involved. The problem of availability again hangs together with the stability of structures. Particularly interesting in this connection are the experiments of Schwarz.[15] Schwarz showed that the "availability" (*i.e.*, whether or not a part of a structured whole occurred) of a response depended on the structure of the whole in which the response in question was imbedded.

"Availability" of a response refers to the fact that as structures vary in their stability, certain responses can be differentiated from the wholes in which they occur into new wholes more readily than others. The stability of structures conditions the appearance of new responses and so conditions whether in a given situation learning or habitual response will occur.

D. *Trial.* By trial Thorndike refers to the utilization of the incorrect or inadequate responses on the way to the goal. This is a problem of motivation and lies without the limits we have set for ourselves in this paper. One quotation, however, will show how very close Thorndike has come to certain basic Gestalt concepts. "For example a boy confronts the situation—Find the product of 435 and 721. . . . Find the product of 435 and 721 not only starts him on the first step of writing 435×721 or 721×435, but also remains a pervading

[15] G. Schwarz, *Psychol. Forsch.*, 1927, **9**, 86–158. Gottschaldt's experiments already reported are also applicable. *Cf.*, further, R. H. Wheeler, The Science of Psychology, Chap. XI, New York, Crowell, 1929.

element and controlling factor during the remainder of the chain, until some status is attained which announces that the required result is attained and bids him turn his mind to other things" (p. 93). Surely we have clearly here "perception of remote goals," leading to "resolution of tension" and hence "equilibrium." These are basic concepts of the Gestalt theory of motivation.

E. *System.* System concerns "the tendency to make the connections or links or bonds or associative habits which I have represented as the fundamental dynamic features of mental life subservient to certain logical and conventional systems" (p. 94). "On the other hand these systems, from the humblest, such as the alphabet, to the proudest, such as a science or a philosophy, are themselves constituted out of connections." Here Thorndike infers (without any experimental basis at all) that systems are built up of connections. If connections in themselves depend on belonging, identifiability, etc., it should be obvious again that system implies organization.

We have attempted to demonstrate that Thorndike has unwittingly adopted the basic concept of Gestalt psychology. In his book, however, he devotes a considerable section to criticism of Gestalt psychology. Our next section will attempt to demonstrate that this criticism is not valid.

IV. Thorndike's Critique of Gestalt Psychology

The main points of Thorndike's critical attack on Gestalt psychology are the following: (1) To him, at least, it is incomprehensible (p. 122). (2) It is not (at least in the best sense of the word) mechanistic—implying that it is vitalistic (pp. 125 ff). (3) Perception and learning to perceive (*i.e.*, where presumably the Gestalt psychologist has something to say) are of less importance than other types of learning (p. 124). (4) Our knowledge of the central nervous system makes the Gestalt theory improbable (p. 131). The fourth point alone seems to us to be scientifically debatable. All are invalid as the following considerations will show:

(1) That scientists deal with theories without understanding them is commonplace knowledge. Thorndike, however, by admitting in a delightfully frank manner his lack of *ability* to understand the Gestalt theory implies a great deal more than his ignorance of the theory. He implies that since it is not comprehensible to an outstanding authority on the psychology of learning, it therefore must

be vague and perhaps mystical.[16] We very much doubt Thorndike's inability to understand Gestalt psychology. Certainly many contemporary psychologists have been able to understand it. As soon as scientists had themselves acquired the intellectual tools employed by Einstein in his formulation of the relativity theory the number of the original "five" people in the world who could understand him was multiplied to several thousand. For a general understanding of relativity to-day interest, intelligence and application are sufficient. In the light of Thorndike's past attainments it seems obvious that he has the requisite intellect to understand Gestalt psychology.

(2) Thorndike's professed inability to understand Gestalt psychology is undoubtedly the basis for finding it unsatisfactory when compared with other mechanistic theories. Scheerer,[17] in the most careful and scholarly critique of Gestalt psychology to appear recently, criticizes Gestalt psychology for being all too mechanistic and finds Gestalt psychology to be a behaviorism, a psychobiology.

We believe that Scheerer's critique is essentially valid and that the methodology of Gestalt psychology is mechanistic. By a mechanistic methodology is meant any methodology that does not involve unverifiable causal factors. This is all that it can mean in modern science. Mechanism has been loosely used to cover all sorts of positions, *from* the belief that all the events of nature were to be explained as resultants of the movement of material particles, as expressed in the laws of kinetics, *through* the belief that biology should use only concepts which could be referred back to physics, *to* the belief in determinism of any sort as opposed to indeterminism. The first usage could well be called the "mechanistic ideal." Modern physics no longer accepts this mechanistic ideal. The nineteenth century biologist usually affirmed his belief in it and proceeded forthwith to use explanatory concepts (natural selection, Mendelian inheritance) that have no obvious connection with it. Certain fields of physics to-day actually are as removed from the "mechanistic ideal" as Gestalt psychology. In designating Gestalt psychology as mechanistic

[16] Such an implication seems to us an unfortunate one. One can imagine that because of Thorndike's great prestige many a reader of his book has said to himself, "If a man of Thorndike's caliber can not understand this new nonsense why should I even try." This is really unfortunate for the best interests of science.

[17] Scheerer, *op. cit.* This book is critical and comes not from Berlin, but from W. Stern's institute in Hamburg.

we simply imply that it demands that rigid determinism which makes for a healthy growth of natural science. The time seems past when Gestalt psychology can be accused of being vitalistic, mentalistic, mystical.[18]

(3) Thorndike's neglect of perception as a "less important problem for the theory of learning" is hard to justify. Perception seems to us basic to all learning and there is certainly no *apriori* reason why its laws should be basically different from the laws of learning. Indeed, evidence is rapidly accumulating to the effect that the structural principles and laws of the more advanced science of perception do apply to learning.[19] Methodologically one of the strongest points in favor of Gestalt psychology is its demand that generally valid laws be found, a precedent favored by the more developed science of physics.[20]

(4) Thorndike finally regards his theory of learning as "far simpler and more in accord with what the neurones are and can do" (p. 131). If this criticism is valid, it is, we believe, serious enough to give considerable advantage to the atomistic theory. We hold it without validity, however, because we are convinced that neither Thorndike nor ourselves know anything of importance concerning "what the neurones are and can do."

If, after Koehler's theoretical discussion of nervous physiology and the subsequent striking verification of a critical side of Koehler's theory by Coghill and Lashley in experiment, Thorndike is still convinced that he knows "what the neurone is and can do" he certainly retains an almost religious faith in the neurology of his youth.[21]

[18] For the loose usage of the term mechanistic *cf.* J. H. Woodger, Biological Principles, New York, Harcourt, Brace, 1929.

[19] We are informed that W. Koehler in his paper before the International Congress of Psychology in Copenhagen, August, 1932, reported striking similarities between certain laws of perception and reproduction. In his eight organismic laws, R. H. Wheeler (*op. cit.*) has made a valuable attempt to find general laws that will be applicable to the whole field of psychology.

[20] *Cf.*, K. Lewin, Gesetz und Experiment in der Psychologie, Philosophischer Verlag, Berlin a. Erlangen, 1927.

[21] W. Koehler, Physische Gestalten, Berlin, Philosophischer Verlag, Erlangen, 1924.

K. Lashley, Brain Mechanisms and Intelligence, Chicago, University of Chicago Press, 1928.

G. E. Coghill, Anatomy and the Problem of Behavior, Cambridge, University Press, 1929.

Neither Lashley nor Coghill set out to verify Koehler's hypothesis, hence

Furthermore, recent researches through the whole field of physiological psychology point to the essential correctness of Koehler's concept of physiological Gestalten.

Psychologists and sometimes even neurologists are apt to forget the exact circumstances that lent the atomistic neurone theory such an appearance of finality. We remind our readers that the neurone theory is nothing more than an hypothesis based on (1) anatomical findings, (2) clinical findings regarding the behavior of individuals with lesions of the central nervous system, and (3) such experiments as physiologists and psychologists can perform on animal and human subjects. The anatomical findings suggest the *idea* of the neurone theory, but by their nature cannot hope to verify it. The sources of error in clinical neurology are so notorious that we need not mention them. The best test of the validity of the neurone hypothesis lies obviously in the experimental method. We will limit ourselves to the field where the experimental method has the greatest chance of arriving at precise decisions, namely sensory physiology. The monumental work of the sensory physiologists (Helmholtz, Hering, *et al.*) in the 19th century gave a tremendous support to the neurone theory. But the experimental situations chosen (for instance, dark room experimentation on the properties of colored patches) were mostly of the sort that would support an atomistic theory. Since the almost simultaneous appearance of Wertheimer's experiments on apparent movement, of Katz' experiments on surface and film colors, and Rubin's experiments on figure and ground, scarcely a month has gone by without the appearance of some new paper which seriously embarrasses the classical theory of perception. Since the classical theory of perception was both the reason for, and to a certain extent the result of, the atomistic neurone theory, these new researches make the neurone theory extremely improbable. The usual practice in science is to invent a new theory, when an old one no longer covers the experimental findings. The neurone theory is *not* sacrosant. Certainly the neurone theory has no particular qualities that give it precedence over any other theories of science. It seems increasingly likely that it will go to the limbo of the one time useful theories of science and disport itself with the phlogiston theory of heat, Galvani's theory of animal electricity, and many others. But whether or not

the agreement is even more impressive. Lashley has admitted that he found events in the central nervous system to be completely different from what he expected.

it does, its position is certainly not assured enough for it to arbitrate between rival theories of learning.

Except for reaffirming his belief in the neurone theory, Thorndike makes little use of it. It is certainly in no place clear to the present writers how the atomistic neurone theory can adequately deal with belonging, identifiability and Thorndike's other new concepts. Such physiological speculation as Thorndike allows himself is concerned with other matters. Finally, concerning the "mysticism" of the underlying physiology we invite the reader to compare the following passage from Thorndike with anything he can find in the theory of physiological Gestalten.

"A neurone modifies the intimacy of its synapses so as to keep intimate those by whose intimacy its other life-processes are favored and to weaken the intimacy of those whereby its other life-processes are hindered. When its feeding, excretory, and conducting processes are going on well, it leaves whatever condition obtains at the synapse undisturbed. . . . The simple avoiding reaction of the protozoa, inherited by the neurones of the brain is the basis of the intelligence of man. The learning of an animal is an instinct of its neurones" (p. 59). On the next page Thorndike says, "This hypothesis is highly speculative but it is not mysterious." We wonder.

V. Summary and Conclusions

We believe that we have shown that Thorndike in his recent modification of his theory of learning has postulated certain conditioning factors which upon further investigation appear as basic postulates of Gestalt theory. An analysis of these postulates shows that in order to be logically consequent Thorndike must admit the organized whole as a basic descriptive unit. We further believe to have shown that Thorndike's criticism of Gestalt theory and his refusal to accept it are based on prejudice against the theory and an insufficient knowledge of its tenets.

We feel that it would be of great value to the science of psychology as well as to the practice of education in America if Thorndike with his prestige and influence in psychology and education would think through to their consequences the logical implications of his new postulates. We are convinced that, whether Thorndike does this or refuses to do it, the science of psychology eventually will. And why? Because psychologists everywhere are uncovering this troublesome "belongingness" that simply will not allow itself to be understood

as something that accrues to originally existent atoms through experience or anything else.[22]

[22] We append a few more recent theoretical works in English which seem to us important studies toward a more adequate psychology of learning.

1. D. K. Adams, A Restatement of the Problem of Learning. *Brit. Jl. Psych.*, 1931, **22**, 150–176.

2. G. Humphrey, Learning and the Living System. *Psychol. Rev.*, 1930, **37**, 497–510.

3. N. R. F. Maier, Reasoning and Learning. *Psychol. Rev.*, 1931, **38**, 332–346.

4. K. Muenzinger, The Primary Factors in Learning. *Psychol. Rev.*, 1931, **38**, 347–357.

5. E. C. Tolman, Purposive Behavior in Animals and Men. Century Co., New York, 1932.

BOOK REVIEW

WHEELER, RAYMOND H., and PERKINS, FRANCIS T. *Principles of Mental Development.* New York: The Thomas Y. Crowell Co., 1932. Pp. xxvi+529.

Written from the Gestalt point of view, this book supplements in an interesting way Koffka's *Growth of the Mind* and Köhler's *Gestalt Psychology.* Although it lacks the charm of style which distinguishes those pioneer works, it is a readable book, and it makes original contributions. It not only applies Gestalt psychology in the educational field, but also presents a more complete and well-rounded treatment of the principles of psychology than either Koffka or Köhler attempted. While the book covers much of the same ground as the senior author's *Science of Psychology,* it treats some topics much more fully. Moreover, there are interesting differences between this book on educational psychology and that of Ogden and Freeman, which is also a product of the Gestalt school.

Whether or not a reviewer is in full sympathy with the point of view of the book he is to discuss, he should, I think, remind his readers as well as himself, that in his selection of points for comment as well as in his appraisal of the work as a whole, he is bound to be influenced by his own point of view, temperament and general intellectual background. This fact does not seem to be sufficiently taken account of in scientific discussions. The senior author of the present volume says in *The Science of Psychology,* that "a scientific investigator should approach his subject matter with an open and unprejudiced mind," but he does not go on to warn the student who is his reader that while this is the ideal of science, it is impossible for any human being, no matter how good a scientist he may be, to approach his subject with a really open mind. At scientific meetings and in papers in our scientific periodicals discussion is usually conducted as if it were agreed that all parties to the discussion are talking on a purely logical plane, and as if a logical criticism should of course be understood by all, regardless of their intellectual background. Moreover, discussion usually proceeds as if on the implicit assumption that there is one right answer to every scientific question, one point of view in psychology which is right or valid, and none other.

As a matter of fact, leaving aside the more personal and emotional

and temporary factors which to a greater or less degree influence even scientific thinking, every scientist who approaches his subject is inevitably prejudiced (using the word literally and not in a derogatory sense) by categories of speech, by modes of thought and by emotional attitudes which are the cultural inheritance of most civilized human beings; by the particular forms of this cultural inheritance which are dominant in his time and in his part of the world; and finally by the more limited types of belief, appreciation, and desire—social and economic, aesthetic, religious and ethical, scientific, philosophical and religious—which have come to mark his own individual outlook upon the world. It is, I believe, one of the greatest accomplishments of modern biological and functional psychology (stimulated and aided by psychoanalysis), to have pointed out clearly the fact of the enormous unconscious influence of such factors on human thinking.

This does not mean, of course, that I think the scientific ideal of openmindedness should be given up—it seems to me one of the most precious products of civilization—but I think we shall attain a much greater measure of it if we keep persistently in mind in our own scientific work, the strong forces working against it in ourselves as well as in others. It seems to me that it is time for psychologists, who more than any other scientists have reason to appreciate the relativity of human thought, frankly to face the great difficulties in the way of straight scientific thinking, in all their discussions freely to admit their own bias as well as the bias of another party to the discussion, to give up the pretense that complete openmindedness is possible and that logic will of course prevail, and to try resolutely to conduct their discussions tentatively and tolerantly.

This discussion of some difficulties in the way of scientific openmindedness is included here, not only because it is needed as a basis for appraisal of the book under consideration, but also for an understanding of the controversy which is still raging over Gestalt psychology. It seems clear that this controversy is fed and kept alive by various emotional factors the existence of which has not received the desirable calm recognition and consideration by various parties to the discussion. So intense and persistent is the feeling revealed in much of the work both of defenders and critics of the Gestalt faith—even in much of the experimental work—that one is driven to the conclusion that back of the scientific issues which are openly discussed lie other issues, temperamental, philosophical, religious—who knows?—which are the more important determiners of the attitudes

assumed, and which are well nigh impossible to deal with since their real influence is probably often unrecognized, and almost always unavowed, even by the scientists who talk most calmly and most logically.

One minor but helpful means to the end of securing objective consideration of scientific issues would be a custom of giving, at the beginning of every scientific work, a sketch of what we might call the real intellectual history of the author. Since even in an ordinary review a brief sketch of this sort would be helpful, let me practice what I preach. Influenced in my early years by an agnostic father, I attended the University of Nebraska where special work in German and philosophy finally made of me an Hegelian absolute idealist and at the same time a strict Titchenerian introspectionist, with, however, a special interest in biology which I discovered was in a state of rapid change. Later, studying at Chicago, I came, gradually, to see fundamental defects in introspectionism and finally in behavioristic structuralism as well. Following, like fellow graduate students, the newer developments in other biological sciences, I became interested in the pioneer work of C. O. Whitman as well as in the work of Child, Herrick, Carlson and others. Teaching has confirmed and broadened the functional point of view acquired at Chicago, and I should probably now be classified as a functionalist, with strong leanings toward the more objective methods, especially in the psychology of the animal and the young child.

Without some such preliminary consideration as has been given to the question of what the scientific attitude really involves, the reader who is not a Gestaltist and who has not followed the details of the controversy that has at times degenerated into personal fighting, will find it difficult indeed to understand the strong feeling, the belligerency, and the intolerance of other views, which is plainly evident beneath the surface calm of the *Principles of Mental Development*. Such a reader, if he is one of those who believe that scientific progress is better promoted by reasonable and tolerant discussion than by fighting (euphemistically called, in intellectual circles, polemics or controversy), such a reader will make allowances for this emotionality as a natural result of many factors including resentment at unfair criticism, will try not to let it blind him to whatever of scientific good the book may hold for him, and will hope that these unquestionably able, sincere and original authors (and their opponents also) may as time goes on be able to achieve a greater degree of confidence in themselves and of tolerance in the face of criticism, even a recog-

nition that it does not necessarily lessen one's own repute as a scientist to admit the common human liability to error and prejudice.

With S. H. Bartley, Wheeler and Perkins have urged elsewhere that arguments which work both ways are invalid in discussing an issue. But we might urge that arguments which work both ways may be about twice as significant as those which work only one way, for in that case both parties to the discussion may need to revise their ideas. This certainly holds true of the "straw-man criticism." The fact that opponents of Gestalt theory have often wrongly interpreted a Gestalt position and then proceeded to demolish that position (and I think it is a fact), does not by any means justify the Gestalt psychologists themselves in continuing to follow that procedure. Yet reliance on this type of procedure has been from the first a conspicuous feature of the Gestalt movement, which has been largely so far a protest movement, and is abundantly illustrated in the present book. I do not think for a moment that the misunderstanding is wilful or malicious, but I do think that emotional factors as well as limited acquaintance with the literature have helped to sustain it; and that it is just as important for Gestalt psychologists in America to try to understand the main developments in American psychology as it is for psychologists representing the various other schools to try to understand Gestalt. There is good reason for failure on the part of Köhler and Koffka to understand the American psychological scene in the light of its peculiar history; but one might expect a much better understanding from psychologists brought up and educated in this country.

Let us take just one example from Wheeler and Perkins of this straw-man fallacy, and see if we can make some progress in understanding the reasons for disagreement. It would seem to me amazing that a Gestalt psychology appearing in 1932 should still betray so fundamental a misunderstanding of the so-called "trial-and-error-theory" of learning in American psychology, if I did not remind myself, first that the Gestalt critics have as a target needing demolition certain widely popularized but somewhat crude pioneer versions of the theory, in particular those of Thorndike and of Watson; and second that contemporary critics of Gestalt have, as far as I know, been content merely to call attention in a surprised way to the mistakenness of the Gestalt attack without pointing out in any detail its weakness, and without specifically citing better sources of information. In the limited space of a book review it would be rash to hope to accomplish this task in a thorough and adequate way, but I shall

see what I can do in the way of a preliminary clarification of the situation.

On page 356 Wheeler and Perkins say, "The error of the scientific theory leads to the illusion that learning, *in a scientific* sense, proceeds by trial and error." . . . "The trial and error conception assumes that the learning process is, at first, random, and that correct responses are originally made by chance. This means, in turn, that if progress is to occur, correct responses must be selected and wrong ones eliminated. If they are to be selected and eliminated, agencies of some sort must do the selecting and eliminating. Accordingly, the supposed agents are found in the stamping-in effect of satisfaction, attending success, and in the stamping-out effect of annoyance, attending failure. This latter theory is known as the *Law of Effect,* and will be inspected shortly."

Let us briefly examine this statement, item by item. First, the statement that the conception assumes random responses originally made by chance, is misleading in that it gives the impression not only that this is the orthodox statement, generally subscribed to, but that it is meant quite literally. Actually there is no reason for assuming, unless one is looking for and wishes to find weaknesses, that even Thorndike and Watson, the pioneer proponents of "the" theory, whose early theories are persistently attacked by Gestaltists, believed the "random" movements of which they spoke to be *completely* chance occurrences, "having no direction and no remote end" (357). Thorndike and others did undoubtedly overemphasize the relative lack of order of an animal's responses in a problematic situation, and were not careful enough to define their terms. But Thorndike's realization of the fact that the animal's reactions, far from being chaotic or purely chance, are relevant to the situation, is shown in his statement in *Animal Intelligence* (1898, p. 45) that "the cat does not look over the situation, much less think it over, and then decide what to do. It bursts out at once into the activities *which instinct and experience have settled on as suitable reactions* to the situation" (italics mine). Watson, mechanist though he was and is, was quick to attack Thorndike's use of the phrase "accidental connections" (*Behavior,* 1914, p. 259), saying that when a stimulus arouses activity in a receptor "there is just as orderly a progression of events then as later when the habit is formed."

Other psychologists early raised objections to the use of the words "random" and "chance," among them Carr who has all along, in saying that the random character of the "attack" has been exag-

gerated, pointed out that the successful movement or the solution of a problem is a chance occurrence only in the sense that the exact time of its occurrence can not be predicted. Following Dewey in his emphasis on the problem-solving character of learning, it has been for many years, I believe, the general custom in American psychology to consider the existence of a problem as the basic and underlying factor in all complex learning, and implied in this usage, if not explicitly stated, is the recognition that the problematic situation, involving a motivating need, gives direction to the activity from the start. It is recognized that in much learning, especially in ideational learning, this direction may be highly purposive and highly intelligent (though the *word* "purpose" is often not used because it is regarded as ambiguous). It is especially to be remarked that Wheeler and Perkins do not even mention this fundamental aspect of "the" trial and error theory in this country. In their urgent arguments against "chance," the Gestalt psychologists have certainly been fighting a straw-man.

"If they are to be selected, agencies must do the selecting." This of course is a statement phrased to express the *logical consequences* of *Thorndike's* early pleasure-pain theory, called the "law of effect" in Wheeler and Perkins (note the italics). It should be pointed out first that Thorndike himself at the start recognized serious logical difficulties in his statement of the theory, and argued against an interactionistic interpretation. Second, the theory as prevalent before Thorndike's work appeared and also as stated by him was widely attacked on logical grounds by many other psychologists. Of the various restatements by Thorndike and his associates which followed, not all were conspicuously successful from my point of view, and few can be compared in cogency with the formulations of the same general point by Holmes and Carr, but the main consideration now is that the theory in the crude form in which it is stated by Wheeler and Perkins is, so far as I know, simply not a theory to be reckoned with when dealing with American psychology. It certainly represents a common popular conception, often encountered among students and needing careful analysis in a class, but it is a straw-man as attacked by Gestalt psychologists including the authors.

Now, as to the principle of frequency which in a particular form is assumed to be an essential part of "the" trial and error conception, space forbids the discussion that is really demanded. It should, however, in the interests of fairness be pointed out that even Watson, while he thought frequency of very great importance, did not think

that it alone would explain the selection of successful movements (the term "selection" used figuratively, of course) and that in the paragraph of his 1919 book in which he discusses "attempted explanations of the process of fixation" he says, "It should again be emphasized that these are little more than mere speculations." The importance of frequency as a factor was early questioned on theoretical grounds by Thorndike, Carr, and others, and experimental work by Peterson and Kuo, to cite only those whose names occur to me offhand, afforded suggestive evidence before the American days of Gestalt psychology, that frequency can not have the importance Watson attached to it. Carr has for many years urged that frequency is one condition not for the retention but for the elimination of certain movements in perceptional-motor learning, *i.e.*, movements that tend to retard or disrupt the process, and Knight Dunlap is well known for a similar position.

Yet although before *Gestalt* appeared there was a healthy questioning of the importance of frequency, its rôle in the learning process has not yet been entirely settled, as Wheeler and Perkins imply. Certainly frequency or repetition of the learning situation, *i.e.*, "practice," is necessary for much learning, as they admit, and this is one of the senses in which the word has been used. Although any psychologist will admit that no event ever occurs twice in exactly the same form, and although there is very interesting evidence against the hypothesis that specific repetition of a particular movement is essential to fixation, a psychologist as a scientist should be very cautious, in the absence of exhaustive study, before assuming that such repetition can *never* be a factor, and that the thing is settled once and for all. But perhaps the chief factor in the authors' straw-man theory of frequency is their assumption that it is *mere* frequency or *pure* frequency, that is being talked about. It is safe to say, I think, that this naïve idea has not been entertained among American psychologists.

There is not space to go on here even with this very sketchy analysis of "the" theory of "trial and error" learning. I must, however, point out that the authors' assumption, implied throughout their book, that the "errors" in the process are regarded by all "behaviorists" as stupid, blind, and, somehow, needless, mistaken, or wrong, is quite unjustified. Watson himself in *Behavior* raised objections to the already traditional name for this type of learning, suggesting instead "perseverance method"; and the point that the "errors" or movements which are "eliminated" in the course of

learning, are in fact essential and most important aspects of a typical learning process, has been urged with particular cogency on the basis of extensive experimental work, by Carr and others. The attitude toward "errors" assumed in this book reminds one of the unsympathetic attitude toward white rats which Koffka has deplored in Watson, and the heartless starvation of cats sometimes attributed to Thorndike, who in his exuberant and arrogant youth, to quote his own later phrase, called certain acts of animals "stupid." This attribution of inhumane feelings to mechanists is really on about the same plane, it seems to me, as the one-time supposition of certain good members of the W.C.T.U. that all who opposed prohibition were addicted to the excessive use of whiskey and most likely to other vicious indulgences. But I do not mean to be facetious—mine is the perfectly serious point that strong personal dislike of a system, a man, or his extravagent mode of writing, is likely to predispose anyone to such misinterpretations as this one of Wheeler and Perkins and other Gestalt psychologists regarding "errors," and should be taken into account as a source of error.

This unrealistic treatment of "the trial-and-error theory" in America is not an exceptional instance of the straw-man fallacy in the present book, which is so permeated with underground feeling against mechanism in general and mechanists in particular that instance after instance could be taken up and analyzed as these few aspects of the "trial-and-error" theory have been. The main difficulty is that the authors have clearly taken certain opponents—always structuralists, and usually not of the most recent generation—and treated them as if they represented American psychology, whereas actually there are and have been varying points of view in this country and a healthy difference in aim and theory, not only as between the two main forms of structuralism, introspectionism and strict behaviorism, but between these and various functional groups, as well as within the ranks of the latter. Merely to mention a few of the more striking straw-men still (in spite of intelligent if irritated criticism) brought forward in this book, there is the quite misleading statement of what personality is "generally" supposed to be in this country (p. 217); the idea that American psychologists believe in a literal one-to-one correspondence between stimulus and response, and that this belief is a part of the constancy hypothesis (pp. 378, 391); the idea that they believe isolated parts are put together, and that the subject in "motor-learning" "combines separate movements previously made"; and finally (17, 267, etc.) the supposition that

American psychologists believe there are quite isolated simple stimuli, as well as more complex details of a situation, which have value *in their own right* (378) regardless of other factors. The last assumption seems to me particularly bad—certainly I cannot remember that I have ever, even during my long past Titchenerian days, encountered such an idea.

In spite of the fact that the straw-man fallacy and other fallacies, exaggerations, and unconscious misrepresentations keep cropping up throughout the book, and in spite of the one-sided and dogmatically absolutistic tone of much of the discussion, which greatly lessens its value especially for students, I find a great deal to commend, and only regret that my plan for this review precludes special consideration of separate chapters. The chapters on heredity and the discussion of tests I think are excellent. There is a sensible and suggestive qualitative treatment of personality, although unfortunately much important quantitative work, by such men as Gordon Allport and Goodwin Watson, is not taken into account.

I find especially interesting the part of the last chapter which deals with "student-centered schools" and "future needs in the light of psychology." Here is repeated the plea for learning through creative response which has been emphasized throughout the book, an emphasis with which I am in full sympathy. Why the authors do not acknowledge indebtedness to John Dewey, in this chapter especially, is hard to understand. His psychological theory has not been popularized but his educational doctrines have, and the direct or indirect influence of Dewey's ideas seems to me to be evident throughout the book. The only reason I can find for the failure to mention him in the body of the chapter is that he is not a Gestalt psychologist. While I probably disapprove of lock-step methods in education as strongly as the authors, I cannot agree with their implied assumption that "education is not adequate because it is not based on sound psychology" (p. 505). The methods we deplore were features of the schools long before "mechanistic psychology" was born, and are doubtless still due to a large extent, as they were a hundred years ago, to complex causes in the social, physical, and economic environment.

Certain social ideals of the authors, as set forth in several parts of the book, I also entertain whole-heartedly. But I can see no inherent connection between these ideals and Gestalt psychology. No group of psychologists has a monopoly on ideals. Ethical conduct, moreover, is taken account of by other systems of psychology,

and, many would argue, more adequately by some schools than by Gestalt psychology.

One highly commendable general feature of this book is its insistence on the monistic approach. In my belief the traditional commonsense dualism, taken over by men of science, is the greatest present obstacle to scientific progress in psychology; but so firmly intrenched is it in modern thought, that it is only by the most heroic efforts that even now a scientist can bring himself to think consistently in scientifically monistic terms. Dewey, James in his later years, and Carr notably in more recent decades, have argued clearly and well for this conception of the nature of mental life, a position forced upon us, I believe, by recent trends in science in general but especially in biology, including psychology. But the dualistic view of mind still lingers, especially in applied fields such as educational psychology, which often lag behind the main developments in the science; and hence the fresh aid brought by the Gestalt school, and by Wheeler and Perkins in this book, is welcome. It must be said, however, that in their statement of this position they make the mistake, so common in past Gestalt psychology in general, as well as in the present book, of assuming that this monistic-functional conception of mental activity is a recent conception, and an exclusively Gestalt conception. There has been a strong and continuous functionalist development in this country since the early nineties, with Dewey as the chief historical leader (his 1896 article was a landmark in American psychology), and with certain members of the Chicago school, notably Carr, as the chief contemporary representatives. While producing a great quantity of experimental work, the functionalists have written few general books, and, so far as I know, no popular books or articles. Others than Gestalt psychologists lack familiarity with their position.

Our thanks are especially due the authors for being willing, in the face of of Köhler's discouraging statement that exact definitions have no place at the beginning of a science founded on experience, to give definitions of many terms and hence to facilitate the meeting of minds which is all but impossible in scientific discussion unless each party knows exactly, or I should say approximately, what the subject of discussion is. But new definitions in this book are too general, from my point of view, to be very useful in general psychological thinking, however functional that thinking be. For example the term "insight" is applied to a very wide range of phenomena, from quite simple acts of sensory choice to very complex aspects of symbolic behavior. As I pointed out in my *Child Psychology* (p. 230), the

sort of behavior which appears in the shades-of-gray experiment and many others, must be a very common feature of mental life which involves repeated responses to sensory situations, since relative intensities and sizes are constantly changing. All perception is of course relative. If we apply the term insight to such a wide and varied range of phenomena as the authors do, then we are left without tools for dealing with many highly desirable finer distinctions, unless we use other subordinate concepts to take account of those distinctions. The term as used in this book becomes, in my judgment, so general as to have relatively little meaning as a functional tool of thought.

The same thing may be said of the "law of least action," and of such concepts as maturation, emergence, differentiation, organization, and "wholes," as used in this book. These represent perfectly legitimate generalizations, I should say, and may be helpful *on a certain level of analysis,* for example, let us say, for the purposes of communication with scientists in other fields or with educated people in general. But as psychologists seeking as deep and flexible and useful an understanding as possible of the human nature which we believe it is so important to understand if not to shape, we need a more careful analysis, we need more finely discriminatory and more delicately functional tools for our research, than these general concepts afford.

Personally I do not believe that there is only one right system of psychology, one right answer to every question. This is a common position, but it savors of the absolutism in which I as a functionalist have no remaining faith. A certain point of view, a certain choice of materials, a certain level of analysis, may serve truly and well the needs of one person or a group, and be applied with satisfaction to the group and with results useful in the general field. For other people and groups, with other interests and purposes, quite other systems may be suitable and prove fruitful and useful in different ways. The Connecticut valley may be viewed on foot, on horseback, from an automobile or from an aeroplane; by a little child, by an old resident, by a romantic individualist, by a Marxian socialist—and the resulting descriptions, while strikingly different, will all, if sincerely given, have their own interest and their own truth. To apply the figure in psychology, I believe that all the main systems of scientific psychology represent perfectly legitimate positions. Whether or not the results of a particular systematic approach are considered useful will depend upon the social and intellectual background, the temperament and the training of the person judging.

The new Gestalt psychology—new certainly in many respects,

although youthfully inappreciative of the important contributions of its parents and relatives—this new psychology can perhaps be better understood when thought of in the light of its setting amidst the confusion of the post-war period. It is perhaps one aspect of the more or less romantic reaction which has been increasingly felt in recent years in intellectual circles, against war-time disillusionment and hard-boiled cynicism, Menckenism in morals, realism in art, determinism in science and negation in religion. But whatever degree of truth this suggestion may have, there are signs that Gestalt psychology will be more than a temporary fashion. There seems to be a real place for it here, and after the tumult and the shouting have subsided, the new school may take a normal place along with other vigorous and rapidly developing psychological groups in this country. If it is thus to take root, its leaders, I should say, will have to become much better acquainted with, and also more tolerant toward, these other groups. Koffka has made an excellent beginning with his recent thorough study of Tolman's "Purposive Behavior." It is to be hoped that Wheeler and Perkins, able and sincere as they are, will follow up the present very interesting book with other work that will reflect a growing maturity and breadth of view.

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NOTES AND NEWS

Norwegian Psychological Association

The Norwegian Psychological Association was organized on April 4, 1934. The aims and objects of the Association are very much along the same lines as those of the A.P.A.

The following officers were elected: Dr. Harald Schjelderup, President; Dr. Richard Eriksen, Vice-President; and Aase Gruda Skard, Secretary. The Association may be addressed, care of Psychological Institute, University of Oslo, Norway.

Eighth International Congress of Technopsychology

Word has been received by W. V. Bingham, American member of the governing board of the Association Internationale des Conferences de Psychotechnique that the Eighth International Congress of Technopsychology—or Psychotechnics or Industrial Psychology as it is variously translated—will be held next September in Prague. This congress, originally planned for Vienna, has had to be twice postponed on account of difficulties in the international political situation. American psychologists who expect to be in Europe next September are asked to advise Dr. Bingham at 29 West Thirty-ninth Street, New York, who will shortly have detailed information regarding precise dates and arrangements for this congress. Communications reporting recent technical researches or summarizing contributions made by American psychologists since the Moscow congress in 1931 will be considered for inclusion in the program. Psychologists not planning to attend are nevertheless invited to supply exhibits of newly standardized and validated examinations of general or special abilities, aptitudes, interests or attitudes, tables of norms, improved rating schedules, and similar psychotechnical documents representative of recent advances in technopsychology.

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